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# A PRELIMINARY STUDY OF THE SIGNIFICANCE OF PARTIAL TONES IN THE LOCALIZATION OF SOUND.

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An adequate theory of the localization of sound must take account of three general problems: (1) The physical conditions (extrinsic to the organism) upon which localization depends must be determined; (2) the precise physiological processes involved in such localization must be discovered; and (3) the psychological activities which are concerned must be analyzed and described. Up to the present time no theory has dealt exhaustively with all of these considerations, and the psychological problem has often been practically disregarded.

The extensive experimentation of recent years has rendered it essentially certain that the most important precondition on the physical side of sound localization is found in the relative amplitude of the sound-waves distributed to the two ears.[1] It is also known that, in distinction from their amplitude, the composition of the sound-waves is sometimes of significance in localization. The evidence bearing on this point, however, is lacking both in definiteness and in detail.

The varying intensity in the stimulation of the two end organs with the resultant effects upon the cortex and other ganglionic centers has often been regarded as a sufficient and

[1] For discriminating criticism of important views of modern scientists, see Pierce, 'Studies in Space Perception.'



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self-evident basis for an explanation of the physiological facts concerning localization. Wundt has advanced the idea that tactile nerves are stimulated by the movements of the tympanic membrane and thus contribute to the localization processes. He has made a similar suggestion with reference to stimulations of the tensor tympani muscle.[1] This type of view has been rigorously criticised by Stumpf, who emphasizes, among other difficulties, the undoubted fact that we can correctly localize two simultaneous sounds.[2] E. Mach early suggested the theory that the external ears act as resonators modifying the quality of sounds heard from different directions, and affording thus a criterion of direction.[3] Theories like those of Preyer and Münsterberg have attempted (thus far with limited success) to make the semicircular canals responsible for the physiological phenomena.[4] So far as these latter theories emphasize the release by sound stimulations of quasi-reflex movements of localization, they point to an important and genuine feature of such processes, whether their conception of the physiological mechanism involved be accepted or not.

On the psychological side various factors have been described as contributing to localization: *e. g.*, (1) the immediate consciousness of position in an auditory space; (2) the consciousness of positional relations gained by visual and other supplementary imagery; (3) the consciousness of tendencies to movement on the part of the head and eyes; and (4) apparently, at times, the consciousness of cutaneous sensations from the shell and membrane of the ear and possibly the tensor muscle.[5] Criticism has been much devoted to combating the frequent and careless assertion that we are conscious of the intensity of the

[1] Wundt, 'Grundzüge der physiologischen Psychologie,' Vol. II. (1893), pp. 93 ff.

[2] Stumpf, 'Tonpsychologie,' Vol. II., pp. 53 ff.

[3] Mach, 'Bemerkungen über die Function der Ohrmuschel,' *Archiv für Ohrenheilkunde*, Vol. IX. (1875), p. 72.

[4] *Cf.* Preyer in *Pflüger's Archiv*, Vol. XL., p. 586; Münsterberg, *Beiträge zur Psychologie*, Heft 2, p. 182.

[5] The general psychological question of the reality of an auditory space has been luminously discussed by Stumpf, and in some of its more empirical features is best treated by Pierce. *Cf.* Stumpf, *loc. cit.*, and also 'Ueber den psychologischen Ursprung der Raumvorstellung,' Leipzig (1873); Pierce, *loc. cit.*

sound heard by each ear and that we in this way localize the source of the sound upon the side most intensely stimulated. The fact is, of course, that we are conscious of one sound and one intensity only, and this is referred to some specific spatial position. But the details of the strictly psychological portion of our general problem have been, perhaps, most often honored by neglect. The recent paper by E. A. McC. Gamble is a notable exception.[1]

So long ago as 1875 Lord Rayleigh had made observations upon the localization of tuning-fork tones, which led him to surmise that differences in the quality and timbre of sounds, as heard by the two ears, were of quite as much significance for localization as the mere differences in the intensity of the fundamental tone.[2] In 1879 S. P. Thompson, discussing experiments of his own with the pseudophone (*cf.* the similar observations of Weber, *Berichte der Gesellschaft der Wissenchaften* [Leipzig, 1851], p. 29 — M. and P. Cl.), came to a like conclusion, which he formulated in a later article somewhat more explicitly.[3]

Despite the instructive character of these investigations, it must be admitted that the conditions which were employed are somewhat unnatural, and that in so far, therefore, they jeopardize the scope of the inferences which may be confidently based upon them. In certain of Lord Rayleigh's experiments, for example, two tuning-forks were struck on different sides of an observer, and then, one of them being stopped, the position of the other was estimated. Evidently the conditions produced by sounding two tones and then suddenly subtracting one are very different, both neurally and psychologically, from those arising when a tone is heard as it originates from some single source. In Thompson's interesting experiments an artificial pair of pinnæ were used, enabling an exaggeration of the effects of reflection, etc., produced by the natural pinnæ. That the localization of sounds could in this fashion be disturbed in certain definite ways

[1] Gamble, PSYCHOLOGICAL REVIEW, Vol. IX. (1902), pp. 357–373.

[2] Rayleigh, *Transactions of the Musical Association* (1876); also *Philosophical Magazine* (5), Vol. III (1877), p. 456.

[3] Thompson, *Philosophical Magazine*, January–June (1882), p. 415; *ibid.* (5), Vol. VIII. (1879), pp. 385–390.

is not surprising, nor is it remarkable that tuning-fork tones should show much less liability to modification in this manner than more complex sounds. But, in the nature of the case, such observations cannot furnish a complete chain of evidence as to the differences characterizing the localization of these various kinds of sounds under normal conditions. Notwithstanding the limitations upon the implication of these experiments, they certainly constitute presumptive evidence in favor of the belief that quality changes are of genuine significance for sound localization, especially when taken in connection with such observations as Mach's,[1] and such mathematical deductions as Lord Rayleigh has made, showing that, save for a few positions, there is an extremely small difference in the intensity of the stimulation of the two ears by the fundamental tone of a sound.[2]

Pierce has demonstrated that localization in the median plane, which is notoriously uncertain and inaccurate, can be vastly improved, when complex sounds are used, by learning to note the modification in tone-color, or timbre, which is connected with different positions in this plane.[3] He has also made tests with organ pipes and tuning-forks, which suggest that auditory judgments of distance are affected by tonal complexity, the richer tones tending to be judged nearer than those more nearly pure.[4] In a paper published in 1865, Mach remarked a contrary fact and promised further communications upon the subject, which I have, however, been unable to find.[5] Bloch made certain observations, which appear to agree with Pierce — the fuller, richer tones being judged nearer.[6] On the other hand, the computations of Grinwis, showing the relative intensity of the components of a complex sound for various

[1] *Loc. cit.*; also 'Bemerkungen über den Raumsinn des Ohres,' *Poggen. Annalen*, Vol. CXXVI. (1865), p. 331; *Sitzungsberichte der Wiener Akademie*, Vol. L. (1864), 'Ueber einige der physiologischen Akustik angehörigen Erscheinungen.'

[2] *Loc. cit.*

[3] *Loc. cit.*, pp. 92 ff.

[4] *Loc. cit.*, pp. 163 ff.

[5] *Poggen. Annalen, loc. cit.*

[6] Bloch, 'Das binaurale Hören,' Wiesbaden, 1893.

distances, furnish a theoretical confirmation of Mach's view.[1] The issue is really somewhat ambiguous. Richer sounds may ordinarily be judged nearer than those more nearly pure. The upper partial tones of a complex sound may be relatively more prominent when the sound is heard from a distance, and still the total sound effect be poorer and less full than when the same sound is heard near at hand.

The extended experiments recently carried out in the Psychological Laboratory of the University of Chicago showed conclusively that persons totally deaf in one ear could localize sounds of sufficient complexity with considerable accuracy, whereas approximately pure tones could not be localized at all.[2] The localizations were evidently based upon the modifications which the partial tones of complex sounds undergo, when the position of the sources of the sounds is changed relative to the ear. The introspective evidence offered by the observers in these tests confirmed perfectly the objective results in pointing to this explanation. It is interesting to note in passing, in connection with Pierce's observations upon median plane localization, to which reference has already been made, that in the Chicago experiments certain of the persons deaf in one ear distinguished front and back with distinctly greater accuracy than the normal subjects. It will be remembered that for normal persons the intensity criterion is for points in the median plane peculiarly ambiguous. But quality differences are relatively reliable, if one has learned to employ them.

The observations reported in this paper constitute an immediate outgrowth of these several previous discoveries and form an effort to begin the systematic investigation of the part played by the partial tones in the localization of sound. The positive outcome of the work thus far is largely methodological in character and bears most immediately upon points (1) and (3) mentioned in the opening paragraph, and indirectly upon point (2). For reasons which will presently appear, the work is necessarily extremely slow, and the results already attained do not

[1] Grinwis, 'Ueber cylindrische Schallwellen,' *Poggen. Annalen*, 1877, Beibl. 8, p. 443.

[2] Angell and Fite, PSYCHOLOGICAL REVIEW, Vol. VIII. (1901), pp. 225, 449.

seem to warrant a more pretentious title than that I have chosen. The probability of unavoidable delay in the completion of the observations is the justification for publication at this time. Moreover, the implication of the experiments successfully executed seems altogether definite and distinctly significant for the theory of auditory localization.

A solution of the problem in hand evidently involves certain indispensable conditions which the apparatus was designed to meet. There must be (1) a series of sounds of controllable intensity, including (2) at least one approximately pure tone. It must be possible (3) to produce these sounds at any position relative to the observer without his previous knowledge of their location. It must be possible (4) to have all the sounds given at equal distances from the observer. It is desirable also (5) that the distance should be capable of variation, although the present paper does not deal with experiments in which changes of this kind were employed. Not least in importance, as dearly bought experience has taught me, is (6) the absolute prevention of the reflection of the sounds. Working under expert assurance that reflection could be eliminated by properly arranged draperies, I wasted much valuable time indoors, with the result that often tuning-fork tones, when opposite one ear, would confidently be localized as opposite the other. My failure may have been wholly due to unskilful devices on my part, but I certainly question very seriously whether experiments with tuning-forks can be satisfactorily carried on save in the open air. With many kinds of sounds this consideration is of minor consequence. I may mention in connection with these indoor experiments the interesting effects of fatigue which were repeatedly apparent. If one ear were fatigued for a tone, and within a few moments both ears were permitted to receive the sound, the latter would often be confidently localized as opposite the unfatigued ear, or sometimes as in the median plane, depending on the degree of the previous fatigue. Thompson, in the paper already mentioned, remarks a similar phenomenon, but much less extreme than in my observations.[1]

[1] *Philosophical Magazine* (5), Vol. XII., p. 351.

## Apparatus and Procedure.

To meet the conditions named, work was carried on out-doors on windless nights—a deplorably infrequent circumstance in Chicago — rendering the observations very protracted. A

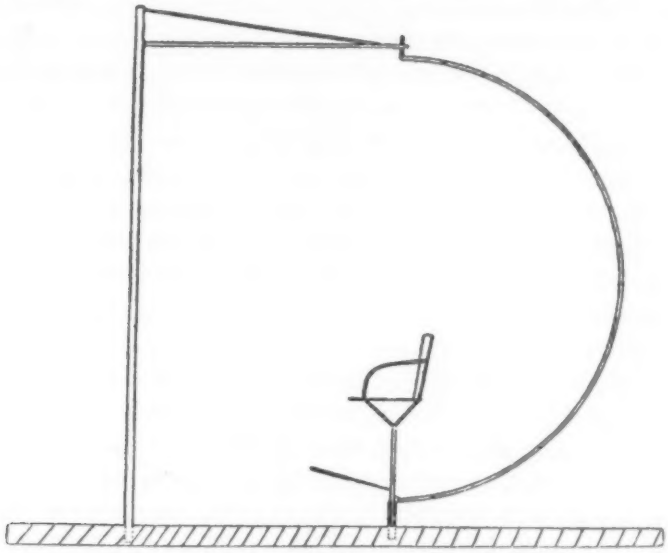

Fig. 1. Platform with Semicircle and Chair.

narrow platform was erected, upon which was mounted the apparatus shown in the accompanying diagrams (Figs. 1 and 2). The upright support (Fig. 1) carries a strong light semicircle

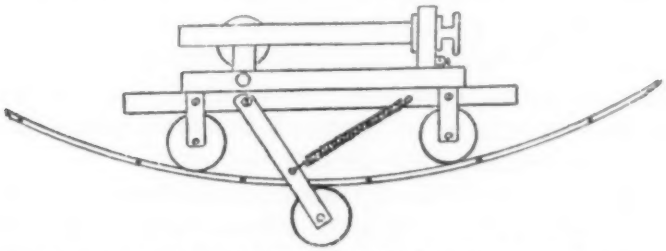

Fig. 2. Carriage with Tuning-fork, Resonator removed.

of steel with a radius of four feet, which is so arranged as to permit a metal carriage mounted on soft rubber rollers (Fig. 2) to travel up and down upon it. The semicircle is marked

off in degrees, so that the position of the carriage can at any time be determined. The carriage is held at any desired height by friction screws acting upon the rollers. The semicircle is very accurately hung and revolves noiselessly. The chair shown in the cut is adjustable in height, and when in use is put at such a point as will bring the line joining the observer's ears into the equatorial plane of the sphere determined by rotating the circle. The chair is mounted on heavy felt cushions which insulate it from any sound-waves which might be transmitted through the semicircle. The platform under the chair is marked off like a compass, so that any position of the circle can be determined. Upon the carriage are fastened the various instruments used for giving the sounds. These are so adjusted that at whatever position the sound is given a constant phase is presented to the observer.

The sounds employed were as follows: From (1) a tuning-fork of 1,000 vs.; (2) a stopped pipe of 768 vs.; (3) a reed pipe of 768 vs.; (4) a bell with a fundamental tone of approximately 2,048 vs.; and (5) a noise made by a telegraphic sounder. It would have been desirable to work with tones which were all of like pitch, but this was out of the question for the time being. It will be observed, however, that all the tones are within the middle range of the musical scale, and that they are quite close together in pitch, two of them being of identical vibration rate. The sounder and bell were operated by closing a noiseless electric contact. The two pipes were controlled by blowing through rubber tubing. The fork requires a somewhat more detailed description. It should be said that the intensity of all the sounds was kept as nearly constant as possible, and that the intensity aimed at was such as to render all of them perfectly distinct, without their becoming unpleasant.

The arrangement for the tuning-fork constituted the most elaborate and most troublesome technical part of the problem. To secure as nearly pure a tone as possible a carefully constructed resonator was made and mounted over the fork. The fork was supplied with a magnet between the tines, and this magnet was then connected with the circuit of an interrupting fork of just one half its own fork's rate of vibration. This is

the device employed by Helmholtz in his celebrated experiments upon vowel sounds.[1] By bridging the spark in the driving fork one secures a tone in the second fork free from all accessory noises of interrupters, hammers, etc. In my experiments the driving fork was kept where it could not be heard, in a house at a distance from the experimental platform. That I thus secured an absolutely pure tone is, perhaps, more than can be confidently asserted. Resonator analysis failed to detect any tone apart from the fundamental, and, so far as concerns my observers, it can be positively stated that they were utterly unable to discern any complexity in the tone. The tone of the stopped pipe was not to them noticeably complex, so that they could confidently detect the overtones, and yet it was not so perfectly pure as the fork. It had the muffled effect characteristic of such tones. All the other sounds were noticeably complex.

My observers sat in an erect position, with eyes closed, but without a head rest. Previous experiments had led me to fear the effect of such a rest, when working with tones of the present character. My subjects were instructed to eschew all tendency to head movements while making their localizations, and I watched them as closely as possible to detect any such movements. Light, open arm rests enabled them to retain an accurate sense of their general bodily orientation and, after a little practice, readily to assume and retain the correct position. They were trained in the nomenclature employed to designate the various positions on the sphere, and in cases of any doubt they were asked to open the eyes and point. Needless to say, on such occasions precautions were taken to move the semicircle and carriage first, so that their position during the experiment should not be thus discovered. The sounds were given for periods of three to four seconds. This time was hit upon as the result of actual experiments made to determine that duration of the stimulus which would permit clear perception, without any feeling of stress or haste, and at the same time avoid tedium and the confusion sometimes caused by wandering attention. The several tests with different sorts of sounds were made as nearly comparable as possible by using the same

[1] Helmholtz, 'Sensations of Tone,' translated by Ellis, 3d ed., p. 399.

positions. This was, of course, not known to the subjects, who were given no indication of any sort as to the point from which they might expect the next sound. Moreover, the order was altered in which the various kinds of sounds were given at the several positions employed.

Of the three men who served as reagents for me one had had no previous practice in such observations, one had had a moderate amount, and one was extensively drilled. The results gained from all of them agree thoroughly in their fundamental implications, although there is naturally some quantitative variation. I place most confidence in the results of the most experienced observer, and I shall devote myself mainly to his reactions. This is the more warranted by the relatively small number of experiments I have succeeded in making under reliable conditions — some four hundred only. The accompanying table (Table I.) exhibits compactly the results of this observer's localizations:

TABLE I.

(REAGENT, J. B. W.)

| Average Error in Degrees. | Sounder. | Reed Pipe. | Bell. | Stopped Pipe. | Tuning Fork. |
|---|---|---|---|---|---|
| Longitude, | 2 | 9.5 | 5 | 30.5 | 53 |
| Latitude, | 7 | 4.5 | 11 | 13.5 | 41 |
| Total, | 9 | 14 | 16 | 44 | 94 |

## RESULTS.

We may say at once, that under such conditions as these — *i. e.*, entire freedom from reflection — there is never any confusion of points in one lateral hemisphere with points in the other, save when one approaches very near to the median plane. Even then this form of error is extremely rare and probably attributable to wandering attention, to accidental suggestion from some extraneous source, or to some similarly irrelevant circumstance. The case of pure tones formed no exception to this rule, and the theories which make the intensity of the stimulation of the two ears fundamental in the explanation of localization, are at least correct so far as concerns the assignment of a sound to one of

these hemispheres or the other. Sounds originating in the median vertical plane are also correctly referred to this plane.

When one scrutinizes the table further, however, it becomes clear that within the lateral hemispheres accuracy of localization appears to be a function of the sound. The average error in localizing the tuning-fork is 94°, which is more than a quadrant. With the stopped pipe the error is less than half as large as this, while with the bell and reed pipe it falls to less than a sixth, and with the noise is at its minimum, showing less than a tenth of the error with the fork. I lay no great stress on these precise figures, yet I have no question but that they indicate the intrinsic nature of the differences in the capacities of localizing these different forms of sound. Certainly the objective record was perfectly confirmed by the subjective assurance of the observers and their promptness of localization. Moreover, when, as in certain special experiments, the sounds were repeated two or three times in quick succession with a very brief duration for each stimulus, the accuracy of the localizations with the *complex sounds* was distinctly improved. This procedure seems to have the effect of making the quality differentia more noticeable than when the sounds are more continuous.

The comparison of the errors in latitude and longitude is not entirely free from ambiguity, because no points nearer than 45° to the poles were actually employed for giving the sounds. The observers did not know that this was to be so, but it makes comparison relatively unprofitable. In the case where localization is most accurate, the errors in latitude are notably larger than those in longitude, as one might anticipate from the standpoint of the intensity theory. In the tuning-fork case it would seem that mere chance might in the main be accountable for the results, with a single exception to be mentioned in the next paragraph.

The longitudinal regions immediately opposite the ears show fewer errors and errors of smaller amount in the localization of the pure tones, than do the regions in front and behind this. Indeed, the most striking difference in the localization of complex and simple tones is to be found in the ascription of the

exact location of sounds to the various points in these lunes diagonally in front and behind. This is in accord with Steinhauser's computations upon the effect of intensity.[1] In the vertical plane, in which lies the line joining the ears, the localizations of pure tones are apparently relatively accurate save as regards height. This constitutes the exception above mentioned and seems to agree with Lord Rayleigh's observations and mathematical calculations, showing that the objective differences in the intensity of the sounds reaching the two ears, which is always relatively small under normal conditions, becomes rapidly less as we move away from the line joining the two ears. With the most experienced of my observers the average error of localization in latitude is nearly four times as large as that of longitude in this region.

Taken in their entirety the experiments seem to indicate that even with pure tones, intensity differences alone are sufficient to enable our confident and correct assignment of such sounds (1) to the median vertical plane, (2) to the lateral hemisphere from which they may chance to come, and (3) the further less accurate and less confident determination that certain sounds of this character belong to the vertical transverse plane of the head. But accuracy of localization as regards altitude in this transverse plane and accuracy in the several regions between this plane and the median plane — accuracy such as is commonly possessed, involving an average error of 10° to 25° — is apparently dependent upon tonal complexity and the modifications in timbre which complex sounds undergo through the changes in the intensity of their partials, when heard from different directions. Localization within the vertical median plane is inaccurate with all sounds, but most inaccurate with pure tones.

The matter can be put diagrammatically as in the accompanying cut, which represents the sphere within which the observer sits. (Fig. 3.) Sounds in the planes *FUBD* and *LURD* can, as the intensity theory requires, be localized with considerable accuracy as regards the *plane* to which they

[1] Steinhauser, 'Theory of Binaural Audition,' *Philosophical Magazine* (5), Vol. VII. (1879), pp. 181, 261.

belong. The exact ***point*** in the plane from which they originate is relatively uncertain, when intensity is the only available criterion. The experiments seem to show with some definiteness that, as we pass from one of these planes to the other, inaccuracy of localization rapidly increases, unless there be definite qualitative differences in the successive sounds. Without such qualitative variations the lune *UEDG* is subject to persistent confusion with the lune *UHDK* and the several points in each lune respectively are subject to gross confusion with other

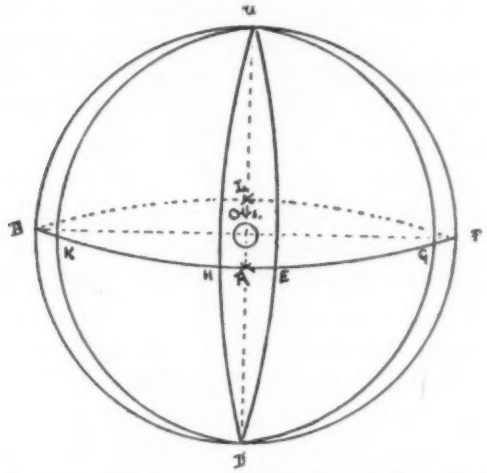


FIG. 3. *F, B, U, D, R* and *L* indicate, respectively, front, back, up, down, right and left.

points in the same lune. Whether the confusion of points in the upper with points in the lower hemisphere is in the case of pure tones notably different in quantity or other characteristics from the confusion of such points with others in the same hemisphere, it is not at present possible to say.

These statements concerning localization as a function of tonal complexity must not be understood as meaning that we are reflectively conscious of this local sign of direction involved in the changing quality, or timbre, of the tones. Sometimes this is noted, but it is not in any way necessary that it should be. Whether or not conscious experience teaches us in childhood to discriminate these varying sensations as having a varying spatial significance, is a question of genetic psychology with which it

is not possible here to deal. Certainly as adults we make the localizations in an almost reflex manner. But the basis of the localizations is found in these symbols reported in consciousness as differences of quality, to which we have come to attach certain space values.

My subjects used much visual imagery in their judgments. My best trained observer seems in his localizations to be conscious of little else beyond such imagery and an occasional tendency to move the head in the direction of the sound. For him to localize a sound means chiefly to get a visual image of the sounding object in the position where he supposes it to be.

The work thus reported opens up the problem which I hope subsequently to work out in more detail. Much fuller observations along the line already pursued are required to permit more confident and inclusive conclusions. Differences in the localization of pure tones of widely varying pitch must be investigated, for the sound shadows involved with such tones and the diffraction experienced by them vary very considerably. It will be necessary to study more carefully the number and character of the partial tones concerned in the differences we have noted. This involves the whole question of relative intensity and pitch in the partials. All one can say at present is that with sounds of medium pitch such accuracy of auditory localization as our common everyday experiences reveal, seems immediately connected with the presence of distinguishable (though not necessarily noticed) partial tones. When such partial tones are absent or very inconspicuous, gross inaccuracy of localization is at once apparent. Detailed information relative to the localization of very high and very low tones is still to be secured. The effect of the duration of the sound upon localizing deserves closer inspection. In connection with several of the points just mentioned the peculiarities of auditory judgments of distance, as distinct from direction, also require more exhaustive investigation than they have as yet received.

In conclusion I wish to express my sincere obligations to Mr. J. B. Watson and Dr. M. L. Ashley, who have given me unsparingly of their time and assistance. I am also indebted to Dr. Warner Fite for assistance in the construction of the apparatus, and to Professor E. W. Mahood for service as reagent.

# THE AFFECTIVE QUALITY OF AUDITORY RHYTHM IN ITS RELATION TO OBJECTIVE FORMS.

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It is difficult, in any appreciation of concrete rhythm, to determine that part of the total pleasure received which is due to the rhythmical form in which the material is cast; for the æsthetic delight which is connected with such an experience is the resultant of a variety of factors, not all of which are purely rhythmical. To make this analysis is, indeed, commonly impossible. The mood is unanalyzable, the object of enjoyment simple. It is the whole experience which is agreeable; and though we are able to say that several sources must combine, since there is present a variety of factors, each of which is a known source of pleasurable stimulation, it does not lie within our power to refer each increment to its separate cause; for the contribution of each factor varies with the aptitude of the individual æsthetic subject, and with the mood and direction of attention at the moment.

The phenomena proper to rhythm are identical in music and verse. The characteristic differences between the two lie solely in the nature of their secondary factors. The rhythms of music are expressed in forms which have rich and pleasing qualities — *tones;* those of verse are supported by forms which arouse varied and beautiful images — *articulate speech.* In the former the immediate sensuous quality of the sound predominates, the ideas suggested are secondary, obscure or lacking; in the latter the images which the words call to mind absorb attention, while the musical qualities of the voice are usually meager or unheeded. It is, therefore, a natural result that in music the formal conditions of rhythm are faithfully observed, while in verse they are transgressed constantly and with freedom.

The description of auditory rhythm as an experience affectively toned involves (*a*) a determination of its purely psychological factors — the modes of feeling which rhythm produces; (*b*) the relation of such modes to types of physical change in the bodily organism, by which the expression of rhythm is characterized; (*c*) the external conditions of pleasurable and painful feeling in rhythm, namely, the elements and relations of the objective rhythm forms which give rise to æsthetic satisfaction and dissatisfaction.

This paper deals with the third of these topics. In regard to the first, a few brief observations. Two factors are unquestionably present in our enjoyment of rhythm. There is what has been called the organic sensation of rhythm, the feeling of response, of pleasurable activity, which is not confined to rhythms actively expressed, but permeates also the appreciation of rhythms which are simply apprehended. There is also the form of the rhythm, which is an ideal content. It is impossible to give expression to the full æsthetic meaning of rhythm in a statement which excludes either of these factors.

The pleasure which such forms of experience afford us is not dependent solely upon an intellectual perception of rhythmical relations, else the peculiar temporal limits imposed upon the forms of succession which support this impression would be irrational; for the formal similarity of sequences which pass in a flash or drag out through hours, to those which we call rhythmical, may intellectually be very readily apprehended, but it is impossible for such measures to give rise in our consciousness to the effect which we call an impression of rhythm. If this were so, in the second place, the impression of rhythm should arise from the apprehension of those visual symbols which represent to the eye musical phrases and metrical speech. But such is not the case; the rhythm is felt by us only when, in some form, we translate those symbols into tones, words or representative movements. The rhythm may be uttered, or sub-vocalized, or the auditory sensations indicated may be called up in imagination as inner speech or song; but in some way, primary or secondary, a sensuous content must be added to the intellectual apprehension of the relations which the audi-

tory sequence presents before any realization of its immediate rhythmical quality can arise in us. And thirdly, under such a conception, the expression in vocal or other motor terms by one who attends to a sequence objectively given should add nothing to the æsthetic effect which that rhythm produces; since it contributes in no way to the formal perfection of the rhythm. Only in the case of one whose auditory or visual perception, or capacity for ideal representation, was undeveloped, could such accessory activities be conceived to add to the pleasure derived from the rhythm; and this solely in virtue of the clearer comprehension of the rhythmical form derived from the kinæsthetic feelings involved in beating out or singing it. Such an hypothesis is self-contradictory, since, if the ideational content be in question and not the feeling of pleasure in the activity itself, the process cannot be conceived to clarify the rhythmical structure, because an apprehension of that ideal form must have been the prerequisite for giving expression to the rhythm.

Nor, on the other hand, can we refer the pleasure of rhythmical apperception or activity wholly to the sensuous feeling and organic reverberation aroused. In some way, clearly or dimly, the form of the rhythm is always felt. The pleasure derives not from the quality of the individual elements and the contrast of weak and strong stimulation or motor discharge alone, but involves also the fact that the rhythm is characterized by formal unity, that it possesses a beginning, a climax, an end, as individual and definite as the quality of the single beat or the constitution of the unit group which enters into it; and this sense of the complete formal sequence is present from the beginning and pervades the whole experience of rhythm. Curtail the series, and its fragmentariness is immediately felt as an imperfection of the rhythmic form; add redundant elements, and the overstepping of the natural rhythmic close is felt in the same immediate way; introduce incongruous forms of temporal or intensive relation, and the discrepancy jars upon the æsthetic consciousness as a violation of the sequence which the rhythmical formation demands. A rhythmical series uncompleted or wrongly executed may haunt the mind for hours or days, until satisfaction is obtained at last by striking the final note or singing the phrase in correct time.

Of these two factors — sensuous feeling and ideal form — the emotional effect of rhythm is due chiefly to the former. Its fervor varies directly with the richness of the sensuous impression and the predominance of the organic reverberation which is aroused. The rhythm which does not move one leaves one cold. Its most intense emotional effects are characterized by violent and widespread rhythmical movement. The ideational content does not in such cases manifest any appreciable development; on the contrary, it may suffer a distinct decline. The apprehension of rhythmic form is an abstract conception, as in that of other formal or mathematical relations. The beauty or dignity of its proportions may arouse our delight, as may a geometrical solution, but this feeling of pleasure is distinct in its origin and nature from that which is characteristically aroused by the reception of a rhythmical impression.

The objective factors upon which the character and intensity of the pleasure in a rhythmical sequence depend may be summed up under the following heads:

1. The absolute rate of succession among the elements of the rhythmical sequence.

2. The absolute intensity of these constituent elements (whether auditory or reactionary).

3. The relation of the absolute rate of succession to the prevailing emotional mood of the moment.

4. The number of elements of which the elementary rhythmical unit is composed.

5. The structural complexity of such units as dependent upon the differentiation of their components.

6. The proportions of the various temporal and intensive values within the rhythmical unit.

7. The form of succession which the elements of the unit group present.

8. The temporal and intensive differentiation of successive rhythm groups.

9. The combination of successive groups into higher rhythmical unities.

To these must be added as sources of gratification in every actual experience of rhythm the following two extraneous factors:

10. The musical quality of the sounds which support the rhythm.

11. The secondary associations which the tones or words of the rhythmical sequence arouse.

1. The absolute rate of succession.

As the very appearance of the rhythmical impression depends upon conditions of absolute rate of succession among the elements which support it, so within the range of temporal relations capable of giving rise to that peculiar experience, the character of the pleasurable impression varies with every change of rate. It is impossible without qualification to say that a progressive variation in one direction is characterized by a continuous increase of pleasure, while that in the opposite direction is marked by a decrease; but it can be said that the varying rates at which the elements of the rhythmical sequence succeed one another have each its peculiar affective overtone, qualitatively unlike those of slower or more rapid rates.

Certain factors which, in a general sense, are elements of the æsthetically agreeable impression do thus increase and decrease as the speed rises and falls. With increased rapidity the definition of the rhythmical sequence, both as regards the segregation of the groups from one another and the differentiation of accented and unaccented elements within the group, becomes more adequate. Secondly, as the rate increases, the limits of the individual group and of higher rhythmical syntheses include a larger and larger number of elements. And finally, in connection with the latter set of changes the differentiation of successive rhythm groups, temporally and intensively, grows more marked. Nevertheless it cannot be said that as the speed rises the pleasure in the rhythmical impression increases. Rhythms of slow tempo and simple phrase may, at any given moment, be vastly preferred to rapid and complexly coördinated forms. The element of absolute rate does not stand alone; its influence can be interpreted only in connection with other simultaneous variants.

Upon conditions of rate depend finally those characteristic qualities by which we describe the various types of familiar

rhythm as gay, sprightly, restful, solemn. These qualities are not attributed to the rhythmical sequence in virtue of secondary associations; they are immediate affections of consciousness due to the pure rhythmical form of the sensuous impression. The rhythm *is* lively or restful; it does not remind us of exciting or quieting things. The quality in virtue of which such terms are applied to rhythmical sequences is describable only in terms of the whole constitution of the rhythmical impression; it is not the rate alone, but the forms of the unit-groups and their succession as well, which produce this peculiar effect; but it is due to the rate, since the whole latter group of changes, except rhythmical melody, varies in dependence upon the rate at which the elements succeed one another. Every rhythmical sequence has such a characteristic quality of gracefulness, dignity, lightness, decision, and the like. The slow are solemn, noble, decided; the rapid are gay, light, graceful. These characters will be mentioned in connection with the number and form of the rhythm group; here it is necessary only to point out that they are fundamentally connected with the factor of absolute rate among the elements of the sequence.

2. The absolute intensity of the rhythmical elements.

The changes in affective overtone due to variations in intensity are much less striking than those which follow upon changes in absolute rate of succession. The same rhythm endures through all degrees of intensive change, while with relatively slight variations in rapidity the affective impression is altered completely. The two phases are comparable with those aspects of sensation indicated by the technical terms 'quality' and 'intensity.' Intensive variations, therefore, are less significantly related to the æsthetic impression produced by the rhythm than are changes in absolute rate. The subordination of this factor has been noted by both Meumann and Ettlinger.

Nevertheless, such intensive changes have specific effects upon the quality of the æsthetic impression, and from moment to moment in our experience may affect it to the extent of determining its sign as positive or negative in a scale of æsthetic values. In extreme ranges of intensity this divergence is most

striking; the quality of a rhythm given in faint, subdued tones is almost incomparably different from that of one of the same structure and tempo expressed in loud and ringing sounds. But within its middle ranges variation in the intensity of the elements is much less readily noticeable, and affects the æsthetic character of the impression in a far weaker degree, than does that of absolute rate. The typical effect of slight intensive changes is to produce a differentiation of phases between the louder and fainter groups of sounds as factors in a higher rhythmical unity. The variation in intensity brings about a new æsthetic construction of material, but is not apprehended in its proper quality at all. This effect is analogous to the disappearance of perceptional differences in the elements of a repeated iambic sequence to which Mach has called attention.

3. The relation of absolute rate to the prevailing emotional mood of the moment.

There is no tempo which can be called pleasing, none which can be characterized as displeasing, apart from the prevailing mood of the moment.[1] It must be congruous with the feeling of the subject in whose consciousness the rhythm appears, if it is to awaken pleasure. In virtue of changes in the subjective attitude, the affective character of a rhythm may be entirely transformed, and that which at one occurrence gave peculiar delight may later become intolerable.[2]

[1] With the exception of such rates as directly interfere with the distinction or force of the rhythmical impression. Such rates are those which lie near to the upper and lower limits of the series which is capable at all of supporting the rhythm experience. These are always less pleasing than rates in the middle of the scale.

[2] James notes this connection between varying moods and specific rhythmical tempos in the following remark: "There is a certain emotional *feeling* accompanying the intervals of time, as is well known in music. *The sense of haste goes with one measure of rapidity, that of delay with another;* and these two feelings harmonize with different mental moods."

But in so harmonizing, the impressions of slowness and haste themselves disappear, and they do so because in each case the periods of objective and of natural organic rhythm have approached each other. The interval of 0.62 second which Vierordt found 'adequate' in the production of an agreeable effect can be interpreted only as the resultant of a plurality of essentially different rates, or as characteristic of a single mood which prevailed during his experiments.

This dependence of particular rhythm forms upon the mood of the moment

These changes in æsthetic quality are not attributable to secondary associations which connect the rhythm in question with former scenes and experiences, the memories of which are incongruous with the present mood of feeling, though the intensity of emotion may be greatly increased by such factors. It is the naked quality of the rhythm which is momentarily displeasing, not that of the associations which it brings to mind. The explanation which I have to offer is speculative. The rhythms which we prefer when fresh, gay, lively, hopeful, when energy is abundant and the amount of movement large, are those which are characterized by rapid tempo and complex integration; they are, in correspondence with the description of our moods, light, graceful, vivacious measures. When we are fatigued, depressed, weak or melancholy, the rhythms we choose are slow of tempo and simple in their construction and sequences; they, analogously, are called restful, solemn, dignified, and the like. In the one case we are attracted by, and spontaneously give expression to, rhythms which involve rapid motor discharges having a wide range of differing intensities and complexly integrated into groups and sequences. In the other case we prefer such as require only slowly repeated discharges which have a narrow range of intensive variations, and present the simplest types of sequence and combination. The conditions of the rhythmical impression are so related to these general subjective conditions that it pleases or fails to do so according as its tempo (together with such other factors as depend upon this) is congruous or incongruous with the normal rate and intensity of motor discharge at the time; and according as the comprehension of the rhythmical form makes or does not make too great demand upon the fatigued or enfeebled attention process. The capacity of any rhythm to please is dependent

for the appeal which they make to the æsthetic sensibility is constantly commented upon by those who take part in such investigations as the present. Comparing two simple forms, one subject says: "These are about equally attractive. In a quiet mood I should enjoy the first; if I were nervous I should enjoy the second." Another says of a certain rhythm: "It indicates hurry to my mind, and would probably, under conditions of mental excitement or great stimulation, be very agreeable. The agreeableness of various rhythms seems to depend upon my state of mind as influenced by other causes."

upon the relation of the attention necessary to apprehend and reproduce it, to our own stock of vigor and mental control at the moment. We dislike those to which we cannot adapt ourselves with success and ease. Dr. M. K. Smith has commented in an interesting way upon the results of such enforced rhythmical adjustment by suggesting that certain forms of social discontent may possibly be attributed to the constant subordination of movements on the part of workmen in various industries to periods imposed by the mechanisms which they operate, which contradict the natural rhythms of the human organism and thereby produce irritable and inharmonious discharges.

4. The number of elements of which the rhythmical unit is composed.

With this factor the characteristic affective differences in the rhythmic impression are commonly connected. Increase in the number of such constituent elements makes the sequence gay, light, sprightly, cheerful; decrease in number renders it solemn, restful, stately, noble. While the number of elements is unquestionably one factor in the production of such impressions of quality, it is only one of a group which combines to this end. The simplicity or complexity of the rhythmic structure is coördinated with the factor of number in affording a justification for the terms stately and noble, sprightly or graceful. The characteristics of smoothness, buoyancy, insistence, boldness, weight, are dependent upon the position and intensive relations of accented and unaccented phases within the group, not upon the number of elements or the tempo directly. Of all measures the spondee is most grave, sober, dignified; the number of constituent elements is small, the proportion of these within any given series which receive emphasis is large, and the tempo, in consequence of this fact, is slow.

The differentiation of trochaic and iambic forms, or of dactylic and anapæstic, from this point of view, is difficult. In each of these pairs the number of elements is identical, and either may be uttered in slow or fast time, thereby transforming the affective quality of the experience and giving to it at will a grave or gay character. At the same time, the absolute

rate at which a rhythmic sequence is uttered stands in intimate relation to the form of structure involved. All double or all triple measures have not the same natural tempo. The rapidity with which it is felt to be appropriate that the sequence should be uttered is affected both by the distribution of intervals within the group, and by the intensive relations of its accented and unaccented phases.[1] The first of each of the above-mentioned groups has customarily been described as bold, flowing, buoyant, while the latter is grave, insistent, weighty; but these descriptive phrases have their origin rather in the position of the accented element of the foot and its proportional time-value than in the number of syllables of which the rhythmic unit is composed. This descriptive classification has been extended — and with equal justification — to prose utterances.[2] Not only the number and relation of elements in the simple group affect the æsthetic quality of the rhythm, but the coördination of these groups in higher unities as well. Dipodic structures are sprightlier than simple sequences, short and common meters than long meter.

5. The structural complexity of the rhythmic unit as dependent upon the differentiation of its components.

The satisfaction which any rhythmical sequence affords is dependent upon a process of individualization which penetrates every part of its structure and gives to each element a functional uniqueness. This differentiation marks the temporal as well

[1] The average duration of the trochaic measure was found in the writer's experiments to be greater than that of the iambic in the ratio 1.000 : 0.791. The temporal values of dactyl and anapæst were almost identical, but the difference which does appear indicates a greater rapidity in the natural tempo of the anapæst than of the dactyl. The amphibrach is marked by a still slower tempo than the dactyl. The series of proportional values is as follows: Dactyl, 1.000; anapæst, 0.997; amphibrach, 1.039.

Ettlinger had previously pointed out this relation of the trochaic and iambic forms, and of series in which the one or the other predominated. He remarks that the Greeks, in virtue of this characteristic difference, called the trochaic '*hesychiastic*' or restful, and the iambic '*diastaltic*' or stirring, and notes the fact that Bach used the former in the choral, the latter in the gavotte.

[2] For the application of concepts of constancy and proportion to prose writing see Schurman, 'On the Length of Prose Sentences,' Univ. of Nebraska Studies, Vol. 1.

as the intensive aspects of the series. It is a factor of which the appreciative subject is not specifically conscious, nor can it be observed by introspective analysis. Its presence imparts distinction, force and character to the rhythm; its absence is marked by monotony and insipidity in the sequence. The notations of music and the so-called poetic forms are never adhered to by the singing or speaking voice, and these departures from the written scale are not made in subservience to the expression of transient emotion, but are constant and universal. The neglect of observing these finer gradations of intensity and duration imparts an inflexible and artificial quality to the rhythm, which is the chief source of the harshness and lifelessness of mechanically produced music. There is properly no reduplication of parts in rhythm. As many temporal and intensive values exist as there are elements in the group. If three members compose it, each of these differs characteristically from both the others in its force and in its duration (or in that of the interval which follows it). Position and function give a different dynamic value to every constituent of the group, the form of which arises not from a separation of its elements into two groups, accented and unaccented, but from the integration of elements each of which is individual and unique.

6. The proportion of the various temporal and intensive values within the rhythmical unit.

These differentiations are not made at random. The unit of structure — and the whole rhythmic sequence as well — manifests definite proportions both of its intensive and temporal values, the maintenance of which is an indispensible condition for the production of an agreeable æsthetic effect. This principle of proportion involves not less than three factors: intensity, duration and position, with their inter-relations. First, the æsthetic effect depends upon the maintenance of just proportions of intensity between accented and unaccented elements. Undue disparity destroys the rhythm, inasmuch as that accented element which is too greatly differentiated from the unaccented members of the group fails to be coördinated at all, and is regarded simply as an unrelated interpolation. This is especially

true when the exaggerated element is a secondary variant from a well-established series of accents, but it holds for simple groups as well.

Of proportions among the various temporal intervals which the group presents we cannot speak absolutely, since the values of these are dependent upon factors of stress or intensity in the preceding reactions.

As regards position, in so far as we can speak of the structure of rhythmical units apart from the form of the whole sequence in which they find place, the initial and final members of the æsthetically satisfactory group are characterized by force, the median element by weakness. Almost all our poetical measures present either initial or final stress; median accentuation is practically unknown. In music the initial stress is observed without specific direction.

But the æsthetic adjustment of relative intensity in initial, median and final members of a group cannot thus be considered apart from the whole rhythmic sequence of which it forms a part; for the specific differentiation of such elements depends upon their position in the verse or phrase of which they are components. The form of the dactyl, for example, may change, as the wave of movement passes along the line, from that in which the final member is the weakest of the group to that in which it is scarcely less forcible than the initial accented element. The production of the perfect æsthetic effect depends upon the maintenance of those particular intensive proportions which are characteristic of each position in the whole series.

The proportions due to inter-relation of the three factors mentioned above involve connections between duration and intensity, between duration and position, and between intensity and position. Of position and duration it can be said, as regards the internal arrangement of the group, only that the values of the intervals must be such as the relative intensity of the preceding reaction demands, and that in the course of the rhythmical sequence they must change as the relations of the reactions vary in dependence upon change of position in the whole series.

In connection with an investigation of the constitution of sim-

ple rhythm forms reported in a supplement[1] of this journal, the quantitative determination of the relative temporal values of accented and unaccented elements necessary to afford æsthetic satisfaction was taken up at a single point. Two rhythmical forms were selected for experimentation, the trochaic and the dactylic. The intensive relations of the rhythmical elements were constant throughout the experiment. The hearer listened for ten seconds to the series of sounds, when the stimulation ceased. Stoppage was always made coincident with the completion of the rhythmical unit. The duration of the single experiment was determined on the basis of reports by the subjects concerned as to the time necessary to apprehend the rhythm clearly and to appreciate its æsthetic worth.

The æsthetic valuation of the various rhythm types afforded by these changes in the durational values of accented and unaccented elements within the group was made by reference to a numerical scale, in which the line of indifference was represented by the number four, and agreeable or disagreeable impressions by departures from this point toward theoretical extremes of seven and one respectively.

Six persons in the case of the dactylic form, five in that of the trochaic, took part in this investigation. The quantitative results are given in the following tables. The figures at the

TABLE I.

(TROCHAIC FORMS.)

| Acc. Elem.<br>Unacc. El. | 1.000<br>0.565 | 1.000<br>0.636 | 1.000<br>0.714 | 1.000<br>0.800 | 1.000<br>0.895 | 1.000<br>1.000 | 1.000<br>1.118 | 1.000<br>1.250 | 1.000<br>1.400 |
|---|---|---|---|---|---|---|---|---|---|
| Subject A. | 4.0 | 4.0 | 4.0 | 3.5 | 3.5 | 3.0 | 5.0 | 5.0 | 5.0 |
| Subject F. | 4.5 | 4.5 | 4.0 | 4.0 | 4.0 | 4.0 | 4.5 | 4.0 | 4.0 |
| Subject M. | 5.0 | 4.0 | 4.0 | 4.5 | 4.0 | 3.6 | 4.0 | 4.5 | 5.0 |
| Subject N. | 5.1 | 5.4 | 5.1 | 5.1 | 4.5 | 3.7 | 5.0 | 4.0 | 6.0 |
| Average. | 4.65 | 4.47 | 4.27 | 4.27 | 4.00 | 3.57 | 4.62 | 4.37 | 5.00 |
| Subject H. | 3.0 | 4.0 | 5.5 | 5.5 | 5.5 | 4.5 | 4.5 | 4.0 | 4.0 |
| Total Average. | 4.32 | 4.38 | 4.52 | 4.52 | 4.30 | 3.76 | 4.60 | 4.30 | 4.80 |

These quantitative results are represented in the curves given below. In the upper drawing the curve of subject H. is represented separately from the rest; in the lower the curves for all subjects are combined in one.

[1] *Monograph Supplement*, No. 17, being 'Harvard Psychological Studies,' Vol. I.

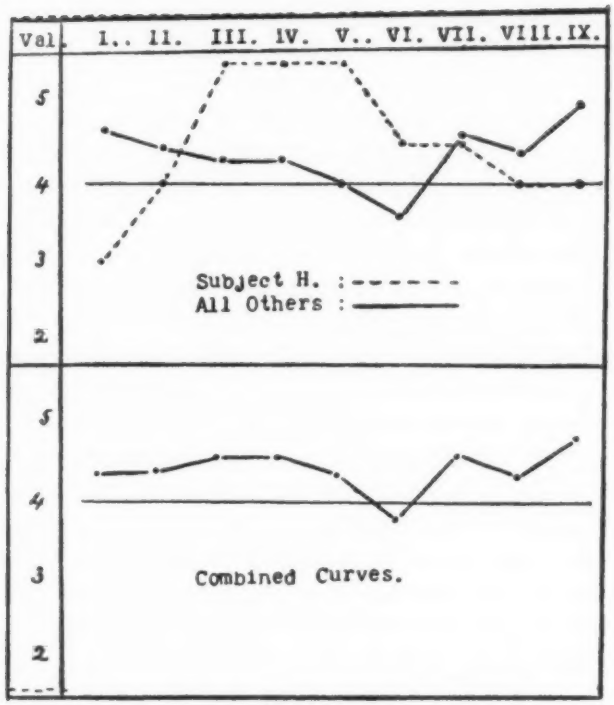


Chart I.

## TABLE II.

(DACTYLIC FORMS.)

| Acc. Elem.<br>1st. Unacc.<br>2d. Unacc. | 1.555<br>1.000<br>0.444 | 1.444<br>1.000<br>0.555 | 1.333<br>1.000<br>0.666 | 1.222<br>1.000<br>0.777 | 1.111<br>1.000<br>0.888 | 1.000<br>1.000<br>1.000 | 0.888<br>1.000<br>1.111 | 0.777<br>1.000<br>1.222 | 0.666<br>1.000<br>1.333 |
|---|---|---|---|---|---|---|---|---|---|
| Subj. A. | 4.0 | 5.0 | 4.0 | 4.0 | 4.5 | 4.3 | 3.9 | 3.5 | 2.5 |
| " C. | 6.0 | 6.0 | | 5.0 | 5.0 | 3.0 | 2.0 | 2.0 | 3.0 |
| " E. | 4.0 | 4.0 | 5.0 | 3.5 | 4.0 | 4.0 | 4.0 | 4.0 | 5.0 |
| " J. | 4.5 | 5.0 | 5.0 | 5.0 | 4.8 | 5.0 | 5.0 | 5.0 | 5.0 |
| " N. | 4.9 | 4.2 | 5.0 | 4.8 | 5.1 | 4.5 | 4.0 | 3.8 | 4.7 |
| Average. | 4.68 | 4.84 | 4.75 | 4.46 | 4.68 | 4.16 | 3.78 | 3.66 | 4.04 |
| Subj. H. | 3.0 | 2.5 | 3.0 | 5.6 | 5.2 | 5.3 | 1.7 | 2.5 | 2.0 |
| " M. | 3.0 | 2.5 | 3.3 | 3.4 | 4.5 | 4.0 | 3.5 | 2.5 | 3.0 |
| Average. | 3.00 | 2.50 | 3.15 | 4.50 | 4.85 | 4.65 | 2.60 | 2.50 | 2.50 |
| Total Av. | 3.84 | 3.67 | 3.95 | 4.48 | 4.76 | 4.40 | 3.19 | 3.08 | 3.27 |

These quantitative results are represented in the curves given below. In the upper drawing the curves of Subjects H. and M. are presented separately from the rest; in the lower the curves for all subjects are combined in one.

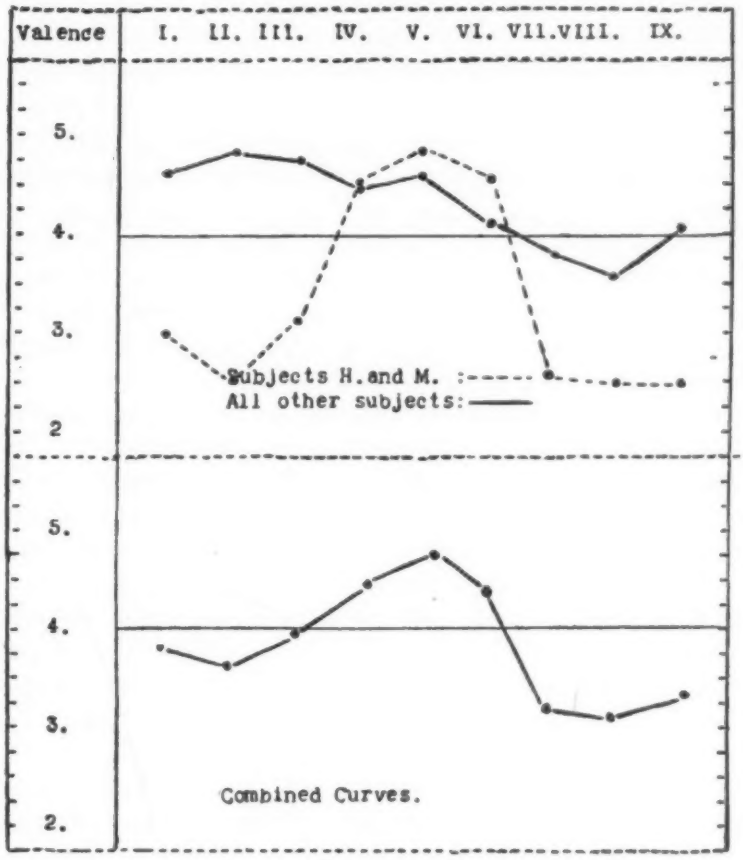


Chart II.

heads of the columns represent the relative temporal values of the intervals; those in the body of the table represent averages of five judgments upon each individual rhythm form.

In the case of each rhythmical form the reports are divided into two sub-groups on account of certain divergences, toward the extremes of the series of values, from the common type of preference which the median range exhibits. In the case of trochaic measures the divergent group consists of one member; in that of the dactylic, of two members, one of whom is that subject who showed divergence in the preceding group. In the case of these two subjects the characteristic preference for pre-

ponderating time-values in the accented over the unaccented intervals persists only within stricter limits than in that of the remaining members of the experimental group. Further differentiation is accompanied by either the appearance of a distinct feeling of disagreeableness or an inversion of the type of rhythm by which an anapæstic form is substituted for the original dactylic.

Æsthetically agreeable rhythmical forms are characterized by non-uniformity in the values of their constituent intervals. The most clearly marked tendency in the estimation of such temporal disparities is to prefer those types in which the interval following the accented element exceeds that which follows the unaccented. The appreciation of forms in which this relation is reversed shows more irregularity. In dactylic rhythm this arrangement of intervals produces a uniformly disagreeable impression. In the trochaic form, on the contrary, the highest positive value appears at the extreme of the descending series, with a second high node adjacent to the point of temporal uniformity. These inverted types have a strikingly different affective quality from those in which the accented interval preponderates; they produce a brilliant, staccato effect, and it is from this novel quality that their agreeableness arises. The upper and lower halves of the scale should be regarded as embodying characteristically different types of rhythm, depending upon specific relations of magnitude among the temporal values of their respective intervals. Within each half of this scale certain proportions are felt to be just and correct in the distribution of these time values; and any departure, of excess or defect, from this ideal relation appears as a mutilation of the proper æsthetic effect of the rhythm. But when one has passed, in either direction, through the zero point of temporal uniformity, and the relations of magnitude in accented and unaccented intervals are inverted, one no longer receives the impression of a distorted rhythmical form which involves a still further departure from the original type, but of one which embodies a different principle of arrangement in virtue of the novel æsthetic effect produced.

7. The form of succession which the elements of the unit group present.

Not only are certain limits prescribed to the absolute amount of variation which may be comprehended within the rhythmical group, and a law of elementary proportion thereby established among the values of its constituents, but the group as a whole must also possess definite proportions in both its intensive and temporal characters. Every unit group and every formal sequence has a characteristic rhythmical melody. The definition of elementary proportions does not describe the nature of the group as a specific rhythm type, for the series of relations cannot be read indifferently in either direction. The form of succession among the elements is of fundamental significance. The same series of intensive or temporal qualities which give a rhythmical grouping having a certain characteristic quality when read in one direction, produce a totally unlike æsthetic effect when read in reverse order. In what this difference consists, one may find it difficult to say, but the unique quality of each form is a matter which immediate experience never leaves in doubt.

The resolution of the factors of this experience is speculative. I should describe it as due to the formal succession of organic tensions involved in the representation of the various rhythmic forms — in other words, to the melodic relations of a series of strain experiences.[1] We may suppose the same simple relations to obtain always between elements of duration, between those of stress, and in the connection of the members of these parallel series with one another. In that case we shall have the conception of definite simple quantities entering unchanged into different melodic series; but such an incorporation is itself a determinant of the nature of the individual element. The æsthetic quality of the single note in music is absolutely dependent upon its formal relation to preceding and succeeding tones in the melody of which it forms a part, and there can be

[1] This view is, in general, in accord with Ettlinger's theoretical discussion, in which the experience is reduced to tendencies toward movement and rest oscillating about points of equilibration in the series. I should prefer, however, to couch the statement in less conceptual and intellectual terms.

as little doubt that the characteristic quality which a specific strain experience possesses depends in a similar way upon the form of the whole series of tensions into which it enters. The value of each (hypothetically) simple quantity in such a series of kinæsthetic impressions undergoes transformation with every incorporation into a sequence having a new form of succession. Upon such formal relations with a series of organic tensions depend the characteristic qualities of the various common rhythm types. These have already been considered in § 5 of this paper. The specific qualities of lightness and weight, of liveliness and solemnity, of buoyancy, insistency or dignity there described are due to the form of the succession of motor impulses and organic tensions severally involved.

The conception of simple, unchanged quantities entering in various orders into characteristically different rhythm types, which the preceding statement has assumed is, however, unquestionably false to the facts. One does not get a trochee by reading an iambus backward, nor an anapæst by inverting a dactyl. The formal succession affects the elements of the series not only by the establishment of laws of melodic relationship, but also by transforming the character of each individual component itself. The relations both of intensity and duration presented by the trochee are different from those which obtain in iambic rhythms, and likewise in triple rhythms the whole series of relations which each type presents is unique and not convertible into other types. This is one of the factors of difficulty in passing readily from one type of rhythmic apprehension or expression to another, which has been largely overlooked in discussions of the matter.

Further, the two series of values, intensive and temporal, influence each other at every point. Not only does the intensively higher element attract to itself a proportionately greater time value, but that factor which is temporally extended is also marked by an (unconscious) increase of stress. We come thus to the conception of groups of elements organized in definitely configured types, and sensitively responsive in every part as the basis of our æsthetic valuation in rhythmical sequences.

8 and 9. The temporal and intensive differentiation of successive groups and their combination into higher rhythmical unities.

These are discriminable aspects of a process which is concretely unitary; they may therefore be considered under a single head. The introduction into a rhythmical sequence of variations from the typical figure is primarily connected in consciousness with a feeling of monotony in the unbroken series, and a desire to avoid the æsthetically displeasing quality which it in consequence possesses. Much less frequently is the motive recorded by the reactor a striving after more definite coördination of the series by the combination of simple groups in higher synthesis. Yet there is every reason to believe that the origin of the feeling of monotony is connected with the absence of those means of grasping larger sequences of elements which the differentiation of alternate measures and the introduction of specific points of phrase-initiation and finality afford; and that the peculiar satisfaction experienced in such differentiated sequences is immediately related to the increased definiteness thereby imparted to the form of the whole sequence.

The delight in such mixed measures is universal, and the tendency to transform the succession of uniform rhythm types by the creation of larger figures is strong and persistent. In the writer's experiments, whenever the subject was left free to choose the form of the sequence, he interrupted the repetition of identical groups by the introduction from time to time of a variant form. Even when required to continue an unvaried rhythm such departures were often introduced. When given a rhythm group which was to form the first half of a composite measure and asked to supply a second, metrically equivalent to it, these reactors — with one exception — differentiated the two subgroups. With the dactylic | | is made to alternate the syncopated form | |, and the like. Of such departures from the prescribed rhythm type the reactors wrote in the following manner:

> "I resort to this form occasionally in order to relieve the monotony of the continuously repeated simple form."

"I try to introduce either a time variation or a difference in pressure" (*i. e.* either a variation in the distribution of temporal relations within the group, or a differentiation into major and minor phases).

"The first measure [continuous iambics] grew tiresome very quickly and ceased to have any quality. The second [iambics with major and minor phases] was, however, distinctly pleasing."

"It seems that to break the monotony after about three [groups] is necessary."

Of four forms arranged in order of complexity one reactor says: "I liked the last combination better than any of the others, though I don't know why."

Such remarks occur in all the subjective reports upon this part of the work.

When different types of rhythmic structure were compared together in pairs, those forms preferred in every instance (with the exception of certain judgments of one reactor) were the more complex, those rejected the more uniform.

All such forms of differentiation derive æsthetic significance from their function as a mechanism of higher synthesis, a method by which unity is given to larger groups of elements as phrases in the rhythmical composition, or a definite configuration imparted to the sequence as a whole. Upon the adequacy of this process and the character of that melodic form which the total series of elements thereby receives, depends more than upon any other one factor the quality of the rhythmical sequence and the delight it affords as a psychological experience.

There remain to be pointed out only certain secondary factors of æsthetic pleasure which enter as inseparable elements into every concrete experience of rhythm, and to the existence and importance of which attention was called at the beginning of this paper. They may be summed up under two heads: *musical quality*, and *capacity for arousing secondary associations*.

The simple musical quality of the tones in which rhythm finds its expression is an immediate source of intense sensuous pleasure. Every rhythmical series of sounds, therefore, becomes a means of greatly increased delight when its component elements are characterized by purity and richness. But the term 'musical quality,' as here employed, involves much more than this. No tone in such a sequence stands alone.

Not only is it rhythmically integrated with preceding and following tones in smaller and larger groups, but it is also qualitatively related to those adjacent members of the series, and depends for its immediate sensuous effect upon the relation in which its quality stands to theirs. It is an element in a melodic as well as in a rhythmical sequence. This functional connection with other tones in a melody reinforces the pleasure-giving capacity of every tone in the series, and greatly increases the æsthetic worth of the whole experience. This melodic element penetrates not only all musical sequences, but the vast majority of utterances of the speaking voice as well; so that in every rhythmical sequence, with the exception of percussive instruments, its presence is to be reckoned with. Thirdly, the musical quality is enriched in still another way by the combinations of tones in harmonies when melodies are complicated by the union of voices or instruments.

The value of the secondary associations of rhythm is still less determinable than that of æsthetic tonal quality, the part they shall play still less predictable. Their forms are two, emotional and intellectual. The emotional associations of rhythm are voluminous and intensely moving. The emotions here in question are not hedonic qualities of the direct sensuous effects of rhythm and musical tones, which may be in the highest degree emotional. They are evoked through memories and ideas awakened by the melody or rhyme which the subject hears. This form is chiefly characteristic of music. In poetry the pleasure of the rhythm is constantly subordinated to the stream of images aroused by the articulate sounds which support the rhythm. The formal perfection of the rhythm is here so constantly broken by the demand for the fulfilment of laws of logical arrangement and expression that the pleasure arising from it is much less important than that which characterizes musical appreciation. It is only when the sense of the verse is momentarily forgotten, and one gives himself up to the music of its flow, that the factor of rhythm becomes pronounced in the æsthetic apprehension of poetical expression. The æsthetic effect of pure rhythm has nothing in common with either the significance of ideational series or the beauty of musical tones.

Its own proper qualities are intense and complex, but these can never be discriminated in the ordinary experience of music and verse. It is only when the intervals of a rhythmical series are defined by limiting stimuli of the barest sensorial quality, and varying only in regard to the intensity of the motor discharges which they originate, that it becomes possible to determine the relation of rhythmical forms to elements of æsthetic value. The attempt to do this in connection with several simple types of variation was made in the investigation reported in the present paper.

# DISCUSSION AND APPARATUS.

## A STUDY OF THE EARLY COLOR SENSE.

In this article I shall give the results of experiments which I carried out on my son during the first year of his life. The results deal with three problems in genetic psychology: (1) The existence of color sensations and their relative attractiveness to the child; (2) the child's visual estimation of distances before he can walk; (3) the way in which the hands are used in reaching before the education of their use has begun. In this paper I shall confine myself within the limits of my experiments and shall not refer to incidents dealing with the color sense recorded in my notes, though these confirm the results. The methods used in experimenting on his color sense were three in number and were arranged to fit in with his mental development. The methods of experiment will be given first in this paper and to each will be attached a short name so that in discussing results they may be easily referred to.

*First or 'Eye Method.'*—For testing by this method some cards were prepared, rectangular in shape, six inches long and four inches wide, and on these were pasted the colored papers used in the experiments. Two of the cards so prepared and of different color were held with their long edges in contact exactly opposite the middle line of his face and about one foot in front of it. The cards were provided with tags at the back so that they could be held without my hands being seen. The proper distance at which the cards should be held was taught by experience. As soon as he fixed his eyes on the edges of the cards that came in contact they were slowly moved apart and the one he followed with his eyes noted. The cards were then reversed in position, the experiment repeated and noted as before.

*Second or 'Grasping Method.'*—This was carried out by using colored crochet-work balls two inches in diameter. The colors of the balls matched the colors of the papers as far as they could be obtained to do so. As with the cards in the previous method, two of the balls were offered to him at one time. The one he reached out his hand toward and touched was noted, the position of the balls reversed and the experiment repeated. These balls gave an exactly similar appear-

ance from all sides and thus were better than ribbons would have been.

*Third or 'Reaching Method.'*—The child sat in a chair having a tray in front of it during the series of tests by this method. For the previous ones he had been comfortably seated on his mother's lap. On to the tray was fixed an apparatus of neutral color for the purpose of holding the cards. Apart from the colored cards the apparatus had no attraction for him. By means of this apparatus cards similar to those used in the '*eye test*,' only reduced in size to three inches long and two inches wide, could be set at a known distance from his eyes and level with them. It was possible also to set the cards to either side of him, but all the experiments dealt with in this paper were made by placing the cards exactly in front of him. To obtain a result a card was placed on the apparatus at a certain distance and the child's reaction to the stimulus afforded by the cards recorded.

The first method was devised by Professor Earl Barnes, the second was an adaptation from his suggestions, balls being used instead of ribbons. The third method is due to Professor J. Mark Baldwin, who has published the results of experiments made on his daughter by this method in his book on 'Mental Development.'

The eye method enabled me to start my experiments on the child's 124th day, he then being exactly 4 months old according to the calendar. As far as I know this is 5 months earlier than any published series of experiments have been started. Before this date repeated attempts had been made to start my tests, but the child was unable to respond to the stimuli offered by the colors till then. The second method was useful when he no longer was content to sit and look at objects, but must touch them for himself, while at the same time he was still too young to start the third method. The reaching method enables us to investigate minutely, not only the child's choice of colors, but his visual estimation of distance, and the tendency, if any, to specialize in the use of either hand. For the purpose of this study I shall include under the term colors the following: Blue, green, yellow, red, dark red, brown, black, white and newspaper.

By the first method I carried out two series of tests, in the first of which all the colors were used. Each color was used with the others 6 times, making a total of 48 tests with every color, this being 216 tests in all. This series commenced on the 124th day and finished on the 155th day. The results of this series will be found in the first column of Table I. The colors in this table are arranged in order of preference, the favorite being placed at the top. The number at the

TABLE I.

| | 1 Eye. | 2 Eye. | Grasp. | 1 Reach. | 2 Reach. | Total Reach. |
|---|---|---|---|---|---|---|
| Number possible. | 48 | 48 | 36 | 105 | 70 | 175 |
| Newspaper. | 45 | | | 74 | 56 | 130 |
| Red. | | | 29 | | | |
| Yellow. | 29 | 32 | 19 | 73 | 54 | 127 |
| White. | 25 | 24 | | | | |
| Red. | | | | 72 | | 122 |
| Blue. | 24 | 22 | 18 | 70 | 51 | 121 |
| Black. | | 22 | | 68 | | |
| Red. | 22 | 20 | | | 50 | |
| White. | | | | 66 | | |
| Green | 22 | | 6 | 65 | 49 | 114 |
| White. | | | | | 47 | |
| Black. | 18 | | | | 46 | 114 |
| White. | | | | | | 113 |
| Dark red. | 17 | | | | | |
| Brown. | 14 | | | 61 | 46 | 107 |

1 reach = 1st series reaching method, etc.

Total reach = total of reach method.

NOTE.—Each line contains only color named opposite it.

side denotes the times each color was preferred, while the total number possible is placed at the top of the column. In the second series, carried out by the eye method, only five of the colors were used. Each pair of colors was used as a test 12 times, giving again a total of 48 experiments with each color. The second column in Table I. gives the results of this second series. The third series of tests were carried out by aid of the grasping method. Unfortunately, I could only obtain 4 colors, and even then the red was not a satisfactory one, which probably accounts for its place on the list. Each pair of balls was offered 12 times, making a total of 36 tests with each ball. This series, the results of which will be found in the third column of Table I., occupied from the 189th to the 203d day. For the fourth series of experiments the reaching method was used, and this series lasted about 9 weeks, and consisted of 105 experiments with each color separately, a total of 840 in all. The results of this series will be found in column 4 of Table I. It should be borne in mind that in the reaching method we have a single color presented separately to the child, while in the previous method a contrast is made by offering two together. The fifth series of tests was really a continuation of the fourth, but the experiments by the third method were divided into two large series so that a comparison of the results might be made. Column 5 of Table I. is a record of this fifth series, in which each card was tested with 70 times, making a total of 560 experiments, lasting over a period of about 6 weeks. Column 6 of Table I. is obtained by adding the

figures of columns 4 and 5, and is thus based on 175 tests with each color, which is a fairly large number and ought to give reliable results. The experiments were terminated on the child's 322d day. The tests by the reaching method had lasted from commencement to close 99 days, giving an average of 14 experiments each day. As regards the experiments, it may be said that the conditions necessary to prevent fatigue, etc., as laid down by Professor Baldwin in his book, were all strictly and carefully observed. From a perusal of Table I. it will be seen that the colors were preferred in the following order: Newspaper, yellow, red or blue, white or green, black, with brown last.

Having arrived at a conclusion as regards the general order of the colors, we shall proceed to study the results in greater detail. Fortunately, we have the tables given by Professor Baldwin in his 'Mental Development' to compare with the results now before us. Very early in the course of my experiments a table was prepared showing the results up to that time in an exactly similar form to the table provided by Professor Baldwin, so that both results could be placed side by side. Table II. is composed of the figures of my results placed under

TABLE II.

| Distance, Inches. | 9 | 10 | 11 | 12 | 13 | 14 | 15 | Total. | Ratio *A/N* |
|---|---|---|---|---|---|---|---|---|---|
| | *R A* | *R A* | *R A* | *R A* | *R A* | *R A* | *R A* | *R A N* | |
| Blue, | 0-1 | 0-4 | 0-5 | 1-3 | 2-4 | 1-5 | 3-1 | 7-23- 30 | .77 |
| | 0-4 | 0-6 | 1-2 | 1-2 | 1-2 | 1-3 | 2-2 | 6-21- 27 | .77 |
| Red, | 0-1 | 0-3 | 2-2 | 1-4 | 1-7 | 1-7 | 5-1 | 10-25- 35 | .71 |
| | 0-5 | 0-4 | 0-4 | 2-0 | 3-1 | 2-2 | 1-3 | 8-19- 27 | .70 |
| White, | 0-0 | 0-0 | 0-0 | 0-1 | 0-5 | 1-1 | 3-0 | 4- 7- 11 | .63 |
| | 0-4 | 0-4 | 1-2 | 1-2 | 0-4 | 1-2 | 4-0 | 7-18- 25 | .72 |
| Green, | 0-0 | 0-1 | 0-1 | 2-1 | 1-4 | 1-2 | 2-0 | 6-19- 15 | .60 |
| | 0-5 | 0-4 | 2-2 | 1-1 | 2-2 | 0-3 | 3-1 | 8-17- 25 | .68 |
| Brown, | 0-1 | 0-2 | 2-1 | 3-2 | 0-3 | 3-1 | 2-0 | 10-10- 20 | .50 |
| | 0-5 | 0-5 | 2-1 | 2-0 | 1-2 | 2-1 | 3-1 | 10-16- 26 | .61 |
| Totals, | 0-3 | 0-10 | 4-9 | 7-11 | 4-23 | 7-16 | 15-2 | 37-74-111 | .66 |
| | 0-23 | 0-23 | 6-11 | 7-5 | 7-11 | 6-11 | 13-7 | 39-91-130 | .70 |
| Ratio *R/n*, | 0 | 0 | .30 | .38 | .14 | .30 | .88 | Total { .33 | |
| | 0 | 0 | .35 | .58 | .38 | .35 | .65 | { .30 | |

**The upper line opposite each color represents Professor Baldwin's figures, the lower line contains my figures.**

those copied from the book I mentioned. The first horizontal line at the top contains the distances at which the cards were placed in the experiments. The line immediately below contains the letters *R* and *A*, which stand for Refusals and Acceptances, respectively. When he refused to reach for a card it was placed among the *R*'s, while if he

did reach after the card it was put among the *A*'s. The first vertical column on the left contains the names of the colors used, each one having two horizontal lines of figures adjacent and belonging to it, the top one being figures given by Professor Baldwin, while the lower one contains mine. In the last vertical column but one are placed the total number of *R*'s and *A*'s beside the total number of experiments got by adding these two (*N*) for each color at all distances. The ratio $A/N$ is got by putting the total number of *A*'s over the total number of experiments, this being found in the last vertical column. In the horizontal line marked 'totals' are the total number of *R*'s and *A*'s for all the colors added at each distance, and below this is the ratio $R/n$, worked out by putting the total number of *R*'s over the total number of experiments for each distance.

It will easily be seen without going into details that there is a fairly close agreement between the two sets of figures, and in the ratio $R/n$ line both show a break in the increasing number as we move from nine inches to fifteen inches, and that break occurs in both at thirteen inches and fourteen inches. My figures were obtained from the complete results of all tests that I made up to the time the table was made, except that experiments which I had made with colors not used by Professor Baldwin were omitted as they were of no use in making a comparison. Bearing in mind the fairly close agreement of our work, we must at the same time remember that the experiments were made under different circumstances. Professor Baldwin's were from experiments spread over six months, and these were apparently sprinkled among many other similar experiments with other objects in view. My figures are based on tests lasting up to that period only sixteen days, and I was experimenting with the colored cards alone. To save my results from the criticism of being hastily arrived at I may mention that not even on the most favorable day did my number of experiments exceed twenty-eight, and then they were always divided up with several hours' interval between, so that no sitting was longer than fourteen experiments; while Professor Baldwin allowed forty experiments in one day. At the end of each sitting our child was always left, like Oliver Twist, wishing for more. Another difference is that Professor Baldwin's experiments were on his daughter, while the subject of ours was a son. Again, the figures would naturally differ, from the fact that the colors used in experimenting are probably different, owing to no comparison having been made. My papers were without gloss and agreed well with the surface of blotting paper used by Professor Baldwin. I have made this comparison to prove that

with care the third method is capable of producing a similar result with two children, and it would be extremely interesting to have several complete sets of experiments tc compare. If we had a few sets of experiments it might be possible to build up a theory of the color sense, which it is impossible to do on these two sets alone.[1]

Having pointed out the agreements in our figures it now remains for me to show the weakness of the tables as presented in Table II. From one point of view the close agreement of the results is purely accidental; not that the method itself leaves anything to accident, but the basis for the table does, if not carefully arranged. It will presently be seen that neither of our sets of figures presents a really valuable result, owing first to the small number of experiments involved and secondly to their incompleteness. It will be seen on referring to Table II. that neither of us had any $R$'s to record for the nine-inch and ten-inch distances, and in the 200 tests which I subsequently made at these distances it was the same — so that these distances are termed safe distances. The ratio $A/N$ is worked out by putting the number of $A$'s over the total number of tests with each color, so that the larger the proportion of safe-distance tests such a total contains the larger will be the ratio $A/N$, from this reason alone, that all the tests offered at that distance are certain to be accepted — every color presenting a stimulus to cause the child to reach over these distances. In looking at our figures we find that Professor Baldwin made three safe-distance tests with brown out of a total of twenty, while white has none at all. Though in the instance just taken to eliminate the safe-distance tests would not affect the sequence of the colors, we can easily see that it might did they run each other close. Besides, it is an example of what occurs throughout both our sets of figures and detracts some value from the tables. To make a table on which we can depend, the same proportion of tests at each distance to the total number for the color must be taken. It is simpler, however, to do as I have done, and take the same number of tests for each color at each distance. The number I used was twenty-five, this giving a total of 175 experiments with each color, or 200 at each distance, or 1,400 experiments in all. The results of these 1,400 tests are arranged in Table III. and this shows no break in the continually increasing ratio $R/n$ which is shown by Table II. Beginning at eleven inches with .07, we rapidly rise as the distance increases till we reach .89 at fifteen inches. The

[1] My attention has been called to a note by Professor Baldwin in *Science*, July 7, 1893, making certain corrections in his figures which, however, only increase the agreement with mine.

TABLE III.

| Distance, Inches. | 9 | 10 | 11 | 12 | 13 | 14 | 15 | Total. | Ratio, $A/N$ |
|---|---|---|---|---|---|---|---|---|---|
| | *R A* | *R A* | *R A* | *R A* | *R A* | *R A* | *R A* | *R A N* | |
| News, | 0–25 | 0–25 | 1–24 | 4–21 | 6–19 | 11–14 | 23–2 | 45–130–175 | .742 |
| Yellow, | 0–25 | 0–25 | 0–25 | 5–19 | 10–15 | 12–13 | 20–5 | 48–127–175 | .725 |
| Red, | 0–25 | 0–25 | 0–25 | 4–21 | 10–15 | 19–6 | 20–5 | 53–122–175 | .69 |
| Blue, | 0–25 | 0–25 | 2–23 | 4–21 | 8–17 | 19–6 | 21–4 | 54–121–175 | .691 |
| Green, | 0–25 | 0–25 | 3–22 | 6–19 | 12–13 | 16–9 | 24–1 | 61–114–175 | .651 |
| Black, | 0–25 | 0–25 | 2–23 | 7–18 | 10–15 | 18–7 | 24–1 | 61–114–175 | .651 |
| White, | 0–25 | 0–25 | 2–23 | 6–19 | 11–14 | 19–6 | 24–1 | 62–113–175 | .645 |
| Brown, | 0–25 | 0–25 | 4–21 | 8–17 | 12–13 | 22–3 | 22–3 | 68–107–175 | .611 |
| Totals, | 0–200 | 0–200 | 14–186 | 45–155 | 79–121 | 136–64 | 178–22 | 452–948–1400 | .670 |
| Ratio $R/n$ | 0 | 0 | .07 | .22 | .39 | .68 | .89 | .32 | |

small difference in the ratio $A/N$ for the colors shows how carefully the experiments need to be made to avoid errors, and, but for the fact that the order of preference as given by this method compares well with the order given by the first and second methods, I should not have so strong a faith in the results.

Fig 1.

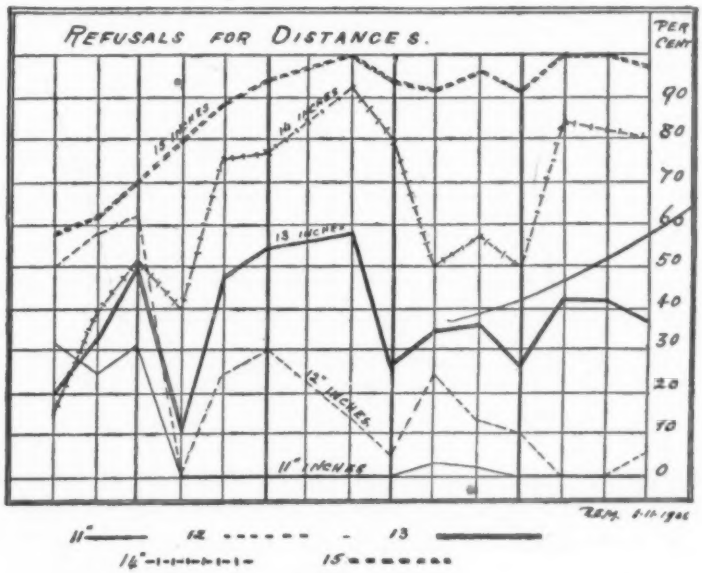


FIG. 1.

We will next consider the way in which the refusals to reach for the cards were spread over the distances, and we will try to see at what rate the child learned to discriminate between them. Fig. 1 is a chart showing the refusals for the distances and the way in which

they increased or diminished as time went on. Each section between the thick vertical lines represents a complete series of 200 tests, there being five of these series. The first line in each section is marked with the proportion of refusals for one third of the series of 200, while the second line bears the proportion for two thirds of that number; the last or boundary line bears the points for the whole series. Thus every section is complete in itself, a fresh start being made each time a new series of 200 was started. Each section was made separately from the others, so that not only could the progress during each series be seen, but also each series could be compared as a whole with the preceding and succeeding ones. Lines drawn through the points on the boundary lines will give curves showing the progress made

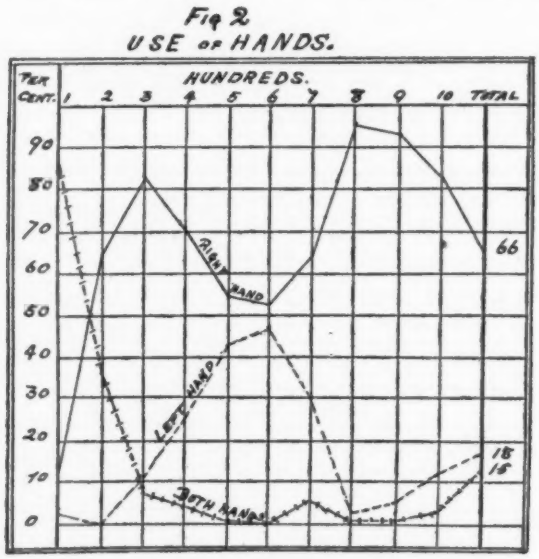


FIG. 2.

during each section, without showing the variations in the sections themselves. It is very noticeable that the curves vary together in a marked degree; a depression in one curve is paralleled by depressions in all the curves at that period. After we once get clear of the first section the curves sort themselves into their proper places, and this chart shows how Table II. has a break in the ratio $R/n$, while Table III. has none. The experiments used in making Table II. are part of the first section in this chart, while the figures for all five sections are used to make Table III. It was during the carrying out of the

experiments represented by the first section that the child was educated into the meaning of inches. After this he had only to learn to more surely inhibit useless movements.

The last part of our study will deal with the way in which he used his hands in reaching out for the colors. I did not begin to keep a record of the way in which he used his hands till I experimented by the third method. In the early experiments he used both hands together more frequently in reaching for the cards, and out of the first hundred *A*'s no less than 86 were made in this way.

In Fig. 2 we have a chart in which the first 9 vertical lines each represent 100 *A*'s, the tenth represents the odd 48, and the last the total of 948. As each 100 *A*'s were completed the total number of right, left, and two-handed efforts were worked out, thus giving a record of the tendency he had to use either hand. If we look at the chart we find that both hands were more frequently used together at first, and that shortly afterwards one or the other hand was used alone and the two-handed efforts almost eliminated. The right hand soon grew in power, and during the third hundred reached its first apex in

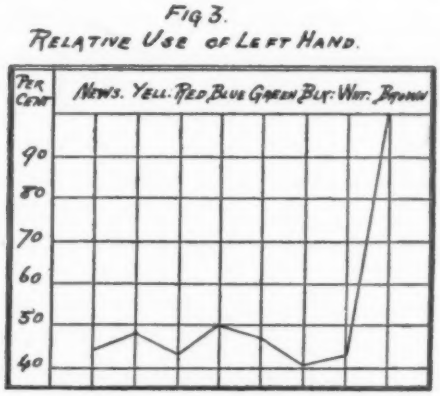


FIG. 3.

the curve. Slowly, however, the left hand is urging its claim in the experiments, and it grows so steadily in the child's preference as to almost equal the right hand during the fifth and sixth hundred *A*'s. From the period represented by the sixth line the left hand loses ground, and ultimately only 6 per cent. of the efforts are with the left hand. The right hand having overwhelmed its competitors—the left hand and both hands together—now makes about 92 per cent. of the effort. The period of greatest use of the left hand was during the

40th and 43d weeks. This coming in and going out of the use of the left hand was also found by Professor Baldwin (see his 'Mental Development').

We have lastly to study the effect of the colors and of the distances on the preference he had at times for the left hand. We will deal first with the effect of color. I have drawn out a chart based on the proportion got by putting the number of times the left hand was used over the total number of *A*'s for that color; by so doing I have deviated from the method pursued by Professor Baldwin in working out the same question. He takes the number of left-handed efforts and places them over the whole number of experiments. The reason why I have taken the *A*'s alone instead of the total number of experiments is that there was probably a latent impulse to the use of one hand or the other, and therefore the *R*'s are uncertain factors which cannot be counted.

Fig 4

RELATIVE USE OF LEFT HAND FOR DISTANCES.

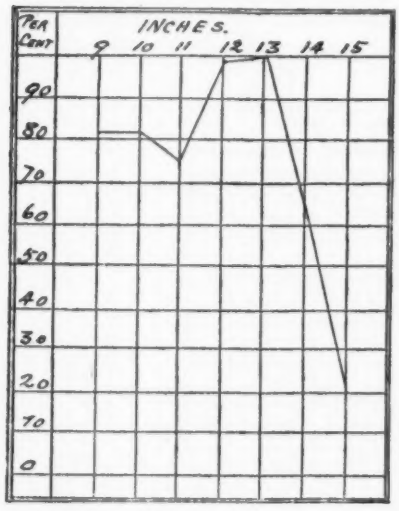


FIG. 4.

When I had worked out a chart by the method described above I found it an ungainly one on account of it being necessary to deal with three places of decimals to show the effect of the color stimulus on the use of the left hand. I therefore made a new chart (Fig. 3) by taking the color with the highest ratio of left-handed efforts as represented by 100 and working out the others relatively to it. In Fig. 3, brown—which had a ratio of .379—is taken as 100, the other columns are then represented by 44, 48, 43, 50, 46, 41, 44, starting from the left of Fig. 3. From this we are obliged to conclude that the colors do not draw out the left hand in a direct proportion to their attractiveness to the child.

It might, perhaps, be objected that the experiments with brown were made during the period of the greatest use of the left hand. This was not so, as the experiments for every column were equally spread over the whole period covered by the tests, and the number with one color never exceeded the number with any other color by more than three.

Pursuing a course, in dealing with the effect of distance on the left hand, similar to that which gave us Fig. 3 for the effect of colors, we get Fig. 4. It will be seen from Fig. 4 that the proportion of responses with the left hand increases up to the 13-inch distance, which is represented by 100, and then declines rapidly. From this it is seen that the effect of distance is not exactly what we should expect, but the fact that various colors were the stimuli probably renders this last question too complex to be answered by the experiments we have been dealing with. To answer this question properly it appears best to take a large number of experiments with a single object.

RUFUS E. MARSDEN.

## EXPERIMENTS IN THE PERCEPTION OF THE THIRD DIMENSION.

I have frequently had opportunity to experiment at random on the perception of solidity, and as the results exhibit some bearing upon the study of the so-called motor factors in perception these results may be described.

The experiment which I have tried many times, and especially when travelling, which affords good opportunities for the experiments, consists in testing both binocular and monocular vision in the way described below.

It is perhaps often remarked that favorable conditions will enable the observer to see in the opposite direction from which the objects are situated the images of any given thing, as if seen through the solid matter toward which the observer is actually looking. Changes of adjustment will affect the results somewhat. That is, adjustment to the distance necessary to fuse images from the real objects in the actual field of vision will prevent the perception of objects with reflected images from the opposite direction. But adjustment to the apparent objects will prevent the perception of the real objects in front of the eyes, so that the effect is actually to destroy for purposes of actual perception the real world at which we look, and we see the imaginary world through it, as if vision could perform the miracle of the Röntgen rays. In this condition a very curious phenomenon occurs. Let me describe a concrete case.

Suppose yourself facing the direction in which the cars are moving, and the light effects are favorable to your perceiving objects reflecting their images from the left to the window panes to your right, so that you apparently see the objects, say houses, to the right on the

landscape or other houses at which you are actually looking. These houses may appear to be variously located to other observers. But for me there is a perfectly uniform action in this respect. If the eyes are adjusted to fuse the images of the houses on my left I see them *through* the landscape or houses toward which I am looking. I may even so concentrate attention as to become absolutely oblivious of the real houses, and the only world that appears to have any reality at all is the imaginary houses or object at which I am apparently looking. Only when I change the adjustment to the images of the real houses or adapt attention to them do I become conscious of them at all, except as vague images in the field of vision. Now what I always notice in the experiment is that the localization of both the real and imaginary world, is definitely related to the different degrees of convergence necessary to determine fusion. The real world, if situated so that this fusion occurs with a greater degree of convergence than the imaginary requires, appears nearer. If not nearer in fact the imaginary world appears nearer. But take the former condition. We have localization proportioned in some way to the degree of convergence, not at which it is at the time, but at which it must be for fusion of images outside the given degree for actual fusion. Thus, although the motor conditions are the same for both perceptions the localization is different. I have called attention in experiments made years ago to this phenomenon. What I wish to note now is a different fact, though it has the same meaning. I have described the binocular effect, and this indicates a difference of perspective for the real and imaginary worlds. But if, while the eyes are adjusted binocularly for the fusion of the images in the imaginary world, I close one eye, the localization, while I remain perfectly still, is the same for both the real and the imaginary worlds. That is, they are seen in the same plane, when monocularly seen. The perspective which is so clearly seen in binocular vision to be different for the different sets of images is now seen to be the same for both. Whether the motor conditions have been changed or not makes no difference in the explanation of the phenomena. If those conditions are altered by closing one eye, and the convergence is probably altered somewhat, the result remains the same. If they are not altered the result also remains the same.

But now, if we move the head from side to side, while keeping one eye closed and using only monocular functions, thus imitating the effects of motion in the cars, we shall notice that the perspective of binocular vision springs at once into effect again. That is, the real and the imaginary worlds seem differently localized and no longer to

lie in the same planes. It is to be noticed, again, that there is no difference of motor conditions for the perception in this case. The motion of the head can hardly be appealed to, as this does not ordinarily affect the results of vision. The adjustment of the monocular system remains the same for both the real and imaginary worlds, and yet the difference of localization is as distinct as in binocular perception. We have a parallax of motion, as we may call it, but this represents only the difference in velocity of the images on the field of vision, caused by the movement of the head, while the muscular adjustment for the focusing of the eyes on the objects is not of the kind to account for the difference of perspective, as that muscular condition remains common to the perception of both sets of images. The deciding characteristic is the parallax of motion, and the effect carries no appearances of being a mere inference. It is as distinct and fixed as any form of localization is, where inference is not suspected or believed. The effect thus has every credential of an organic function of some sort whether we choose to regard it as sensory or motor. It represents in all its characteristics a constitutional function. At the same time, as described and as it always occurs with me, I find no such coördination with what I have understood as 'motor' influences that I could treat it as so caused or as so represented in nature.

In the study of the bearing of such phenomena as I have described on the motor theory of such perceptions I wish to call attention to what seems an inherent difficulty in modern theories couched in the term 'motor.' To call space perception or any other product of similar action 'motor' may be clear or obscure according to the understanding of the term 'motor.' In contrast with sensory functions the term originally represented a wholly unconscious process, and so indicated all the difference from the sensory that the unconscious stands for in comparison with the conscious. 'Motor' thus stood for the initiation of muscular action, a function that never is conscious except in deliberate volition and even then only to the extent of the fiat which finally realizes itself in motion. In the application of the term 'sensory' thus contrasted we were to understand the actual facts of consciousness connected with our primary experiences. Thus the 'sensory' was definitely what we knew and experienced in sense perception. The 'motor' was what we did not know, but what we inferred took place in the central system somewhere. It was of an impulsive character and accounted for the various movements of the organism whether consciously or unconsciously initiated. Now in our modern enthusiasm about 'motor' explanations for various phenomena

we constantly forget two things. First, the fact that 'motor' and 'sensory' come to us with an implied contrast in their relation to consciousness, so that an appeal to the 'motor' invariably suggests an unconscious fact in the result which we are explaining. Secondly, the fact that we do not adequately distinguish between this traditional use of the term 'motor' to denote the unconscious agencies connected with the muscular system, and the application of the term to denote the 'consciousness of motion,' which is a sensory fact and not 'motor' in the old sense of the term. This modern use becomes identical with the 'sensory' and not contrasted with it. Consequently there is little excuse for the controversies which have centered about the various problems of space perception or any other question associated with the so-called 'motor' activities. 'Motor' to describe an activity or function wholly below consciousness is one thing, to describe a fact of which we are conscious is another and very different thing separated from the former by all the diameter of a contradiction.

To make our problem concrete. In the traditional conception of the term, to call the perception of space, or the third dimension, a 'motor' phenomenon would be to deny its sensory character and to take it out of the field of consciousness altogether. Here was where Berkeley created difficulties in his theory. The observer never detected any muscular sensations in his visual act except when straining the eyes and this indicated no apparent effect on space perception. All that he could do was to say or imply that the 'sensations' were unconscious, which was to take the phenomena outside the field of legitimate study. But for us to say that such perception is a 'motor' phenomenon and mean by it that it is either constituted by or caused in connection with the sensations of motion, is to conceive the phenomenon as a sensory one and not to contrast it with the sensory at all. The controversies which have centered about the problem have been caused by the abandonment of the original meaning of the term 'motor' while retaining its opposition to the sensory after that change This is a frequent vice of our psychologists and it leads to both their misunderstanding of the problem as conceived in relation to historical questions and their being misunderstood by others. What we want is a clearer definition of what we are talking about. I have not yet seen any such careful account of terms with a view to avoiding misconception and controversy, and I know no term more in need of careful explication than the term 'motor.'

With the differences of meaning attached to it, I think the reader can anticipate the solution to the issue about the 'motor' perception

of space. If we mean by the 'motor' theory that the function determining space perception is of the order which initiates muscular action, we have an impossibility in the simultaneous localization of different objects in different planes. The 'motor' or muscular functions are seen, in such phenomena as I have described, to be the same or common, but the localization different. The localization varies when the 'motor' action does not, so far as that is conceived in the mechanical terms for which muscular action stands. The 'motor' theory thus seems to have an irrational application. But, on the other hand, if we mean sensations of motion when we say 'motor' and refer space perception to these as its coincidental factor, there will be less difficulty in accepting the doctrine. It is quite apparent in the experiments which I have described that the sensations of motion are the variants with the differences of effects and we may well attribute this difference of effect to the action of those conditions which discover the existence of motional effects in consciousness as the varying factors in the phenomena to be explained. But if we are to import into the issue the implications of contrast with the sensory which the term 'motor' so often has, we only introduce confusion into the problem. We have in fact a case where, when that contrast is excluded, there is identity with the sensory theory at large instead of opposition, so that all the controversies connected with the matter are largely the proverbial dispute about words, except that the illusion is not wholly verbal.

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## DR. BOSANQUET ON IMITATION AND SELECTIVE THINKING.

In my note in the November number of the REVIEW, discussing Dr. Bosanquet's criticism, I expressed the intention of continuing the topic — especially with reference to his criticism of the theory of selective thinking. The criticism in question occurs in his discussion — which was taken up for reply in my earlier note — in the PSYCHOLOGICAL REVIEW for July. Having in mind the general considerations advanced, as to the need of a thoroughgoing application of the genetic method, I make the following points, dealing rather with the adequacy of the theory of selective thinking, the necessity of some such genetic account being assumed.

On p. 386 of his article, Dr. Bosanquet makes the criticism of

[1] A continuation of the discussion in the November REVIEW.

the theory of selective thinking which is embodied in the following words: "The prerogative of reason really is to ascertain without trial and error; that is, without actual working upon things, what adaptation is workable in a given situation. I will take calculation as a very simple instance. When I have calculated how many trees are needed to plant an acre of ground at a given distance apart, the adaptation is fully determinate. There is no room for variation or for selection. Selection of 'actions which work in a given situation' * * * omits the essence of calculation and inference, in short of reasoning * * *. The actions which work, workable combinations, will include the results of theories which work, that is which agree with experience in any form, practicable or cognitive."

This line of criticism is aimed — as I take it — at all views which would attempt to prove that in any act of thought the entire process of organization is one of trial and error; that is, against any view which holds that there is no logical process, or process of inference, in the case of organization in question, which is not then and there due to selection. Such a theory is far from my meaning. In each case of progressive organization — and it is such organization which I have called 'systematic determination,' to which the theory of selective thinking addresses itself — there is the established 'platform' of accomplished organization, and with it the further incorporation of new items or thought-elements into the system. The theory is limited, as far as its application to a particular case is concerned, to the process by which these new elements are secured and assimilated. It leaves open, on the other hand, the question as to the nature and origin of the accomplished platform of organization upon which the process in question proceeds. If we for the moment admit that the incorporation of new elements in the system can only be by a process of thinking which is in some sense 'selective,' we should then have left over the question as to those cases of logical process in which there are no new accretions to knowledge, but simply — as in a case of calculation — the running-through of processes of analysis, substitution, etc., according to normal mental rules.

When we approach this question, there are certain broad and general distinctions to be made. We may ask, first, as to the empirical or genetic growth of these mental rules in the individual — a question which brings up the issue as to whether these modes of organization do not themselves reflect earlier processes of selective thinking, which have become, through association, synergy, etc., self-repeating and in a logical sense analytic. The latter term, 'analytic,' has been used by Wundt and others to mark the distinction which I am here making

(compare the article by Urban in the PSYCHOLOGICAL REVIEW, July, 1897). We may, after adequate investigation, come to the conclusion that the entire furniture of logical procedure in the individual reflects the continued action of selective processes in his individual growth. Or, on the other hand, we may reach the conclusion that there are certain constitutional modes of organization and rules of mental procedure (*i. e.*, Dr. Bosanquet's teleological processes) which are given or implicit in all cognitive experiences as such. In this latter case, the question arises: what shall we say of this saving remnant of logical process, which, in terms of the conclusion just supposed, cannot be accounted for as due to any sort of individual selection?

Having now put these alternatives, as I conceive, in the form most favorable to Dr. Bosanquet's contention, I may say that in my personal view this last conclusion is the correct one so far as individual thinking is concerned. We have, first, a large body of empirical or derived knowledge systems which are due to gradual accretion through the process of thinking in some sense selective; second, we have, at the same time, systematic determinations of knowledge in which, along with this empirical process of selection, certain organizing rules are operative which themselves have not been formulated through the selective experiences of the individual; and third, that there are certain normal processes in a large sense analytical (calculation, inference of certain types, axiomatic argumentation, etc.), which, whatever be their origin, are in substantial independence of the particular character of the contents of experience which are organized in and by them. These last mentioned elements in our knowledge, I prefer to call 'constitutional' rather than to use a more logical or metaphysical term. They represent the constitution of the thinker, and must be set down as in each case seeming to be given in the knowledge processes in which they show themselves.

The criticism of Dr. Bosanquet simply charges upon the theory of selective thinking the failure to account for these last mentioned aspects of organization. So far as knowledge in general is concerned, I fully admit this criticism as applicable to the processes of the individual thinker. But it will be observed that the topic which we are discussing, that of the book on 'Social Interpretations,' is *social organization*. Now, social organization is, to my mind, a typical case of progressive determination by gradual accretions of content to the individual's experience. Its most general mode, the self-thought, is a growing empirical content.[1] The process of social organization which

[1] It is a question, however, whether the categories of unity and identity which characterize the self-thought are not constitutional.

the individual goes through is, in the terms of my theory, tentative imitation with equally tentative selection. But this is empirical process through and through; it is a case in which one would make least reservation in regard to the adequacy of the selection theory in view of the presence of analytical logical processes. Dr. Bosanquet's criticism, therefore, so far as it brings out the inapplicability of the selection theory to such logical processes as calculation and deductive inference, does not apply to the application of it which I make when considering social organization.

I have, therefore, to admit that, in this particular case, he is quite right in saying that, on my view, 'actions which work, workable combinations, will include the results of theories which work, that is, which agree with experience in any form, practical or cognitive.' For all thinking, whether social or not, which involves confirmation — whether it be by experimentation, by simple observation, by induction, etc. — all such theories, so far as they issue in progressive or synthetic determination of knowledge, fall in the class which the selective thinking theory aims to cover.[1]

It is quite true, however, that in the address on Selective Thinking ('Development and Evolution,' Chapter XVII.), the theory is stated as applicable to cognitive process in general, and consequently, apart from the merits of the theory as applied to the case of social organization, it must defend itself with reference to the larger issue which is raised under the second and third heads mentioned above; that is, we should still have to ask whether the established platform, or system of organization which precedes the incorporation of new selective elements, is itself the work of still earlier selective processes, and then we should have to go on and ask whether the residue of process, which seems to be purely analytical and deductive, can, in any sense, be brought under the selection view. So Dr. Bosanquet is quite right in asking for an interpretation of the sentence, "thoughts are the counterparts of former adapted actions" (p. 387), which he quotes from the address mentioned.

[1] That the general position here taken is essentially the same as that of the first edition of the 'Social Interpretations' (written before the address on Selective Thinking) may be seen from the following quotation from that work (1st ed., p. 122; 3d ed., 131): "The general conclusion is, therefore, that there is a great sphere of truth, of selective thinking, of inventions judged true, of mental constructions believed, in which the criterion of selection is all along availability for imitative social assimilation in the growth of the thought of self; and unless in some spheres we be able to find other compelling criteria of truth, we shall have to say the same of all selective thinking." (And in a footnote— "This last expresses the probability, in my personal view.")

Interpretating the sentence — as he does quite properly — as meaning "thought is a phase of adapted principle, carrying on in a new situation the idea which gave a successful conclusion before," he criticises it, saying: "But in this case thought depends upon previous thought of the same kind; the variation becomes predetermined; selection vanishes and with it the prerogative of actual working of things as a test. For all thought whatever is a principle at work in the sense in which calculated action is so, and whether the conditions to be met are materially present, or only present in knowledge, can involve no differences of any kind to the nerve of the process. It is only in trial and error that practice on material things has a prerogative over inference. When we come to calculation and reasoning, the especial import of material action disappears and it becomes one among many forms of cognition, all of which are tested by working in the wider sense." And he actually draws the conclusion in the paragraph which follows, that in all thinking — such as in deciding upon the plan for a new house — we should have to actually perform the experiment; that is, 'to be consistent, you should try the combination in actual bricks and mortar.'

My reply to this is in general that it fails to be in any thorough sense genetic. It fails to allow that past acquisitions, which have themselves been selected, may be so fixed and habitual that the selective processes which they originally embodied are obscured. The realization of a principle at work — to use Dr. Bosanquet's phrase — may be through a series of selective accommodations, and the result at each stage may appear to be a given or original plan of organization when, in fact, it has grown up through a series of selections. In each such case we have to ask, however, what sort of process has actually led to the determination of the principle at the particular stage, and open the question as to whether there is not in the determination a certain amount of 'relative suggestion' due to new selections. We are thus driven back from stage to stage of mental progress, the formal, schematic, or logical ingredient being reduced to its minimum as the elements of the combination are found one by one to have been fixed by actual processes of selection. There is thus, in each case, fusion of what seems to be logical process with its empirically selected content; and we push back the final question to this: What are the original and constitutional modes of organization of our knowledge *when the individual begins his personal career, and which, in the growth of experience, are so and so determined?*

The statement of this problem, in the matter of knowledge, may be illustrated by the latest development of biology in connection with the corresponding problem of the relation of congenital and acquired characters. The old theories were those of Preformism on the one hand, and Epigenesis on the other hand. It was the question in particular cases either of 'congenital characters' or of 'acquired characters,' and one must find one or the other. Recent progress in biology has made it clear that such a thorough-going antithesis is mistaken; that there are no adult congenital characters as such; also, that there are no acquired characters as such; but that all characters include a certain constitutional rudiment or *Anlage*, *whose development is essentially conditioned upon the influences of the environment from the start.* The larger part of the character — whatever that character be — is in most cases a joint product, and the question is that of determining how, in a particular case, the two aspects are conjoined.[1]

This, in my view, is substantially the present state of the question historically known as that between the *a priori* and the empirical in thought. It is no longer an antithesis between two elements set over against each other in the structure of knowledge. It is now a question of *the interpretation of a living organic process which is experience, but which is also function.* We have a growing structure or system of knowledge. It grows by selective assimilation of new elements of content. What are the conditions which determine the mode of organization at each stage of its growth? This is the genetic question — the question which the theory of selective thinking attempts to answer. There remains, however, the question as to what is the original nature, reduced to its lowest terms, of the mental function which takes on these modes in a progressive way in actual experience. The answer to this last question gives a theory of the constitution of knowledge — the endowment factor.

Having admitted so much — that the tracing back of the processes of knowledge to their simplest forms lands us in the recognition of some sort of constitutional endowment — one is, of course, not able to shirk the question as to what this endowment means. And anyone who leans toward a thoroughgoing genetic point of view raises the further inquiry as to the racial evolution of endowment. Here the ground becomes more or less familiar when looked at historically, and the two theories in the field are, of course, those of 'race-experi-

[1] Perhaps I may be allowed to refer to my own statement in exposition of the view of the 'Organicists' in 'Development and Evolution,' pp. 34ff.

ence,' on the one hand, and 'variation,' on the other — that is, *if one be an evolutionist at all.* Once accepting the genetic point of view in race-evolution, we find reinstated for the successive stages of racial progress the same question as that discussed above for the individual's progress; namely, how is it that successive forms of mental organization can have been produced in connection with the corresponding evolution of physical forms? And it is a commonplace to say that the selection theory, in the sense of current Darwinism, provides a way of accounting for this racial progress, as the selective theory of thinking aims to provide an account of the individual's progress. All the way through, however, from the human mind back, let us say, to the protozoan mind, the genetic psychologist, like the general biologist, aims to provide a theory of what he sees without being called upon to suggest a philosophy of the nature of the reality whose successive determinations he sees. The philosophical problem for the evolution series, as for the series representing the individual's development, remains over. But whatever its solution be, it should not be allowed to intrude itself upon the genetic psychologist, or upon the biologist, who pursues the problem through both series with view to the most adequate statement of the facts, and formulation of the methods of their organization.[1]

[1] We should, I think, expect that the completed genetic survey would afford most essential data for sound philosophical construction; and there are signs of the recognition of this resource — signs that the genetic method is forcing its way into metaphysics. In the recent 'Outlines of Metaphysics' of Prof. J. S. Mackenzie, we find the explicit declaration of such a platform and intention, but little first-hand psychology, and further a certain helpless deference to the traditions of a school. On the other hand, in the recent work 'Foundations of Knowledge,' by Prof. A. T. Ormond, we must recognize a book of very different character. It is, in my opinion, a very able piece of constructive work, in which the attempt is seriously made, and with understanding of what the attempt really means, to treat of the origin and development of knowledge with view to the interpretations of epistemology and philosophy. (But that it is *caviar* to the philosophers of the classical logical method may be seen in the review of it by Prof. D. G. Ritchie in *Mind*, January 1892, p. 92, who misses the meaning of the book in all its large features, and devotes pages to fretful pedantry over certain misplaced commas, and Greek accents gone astray. This seems, however, to be a chronic ailment of Professor Ritchie, and does not weigh for much in a country which holds Bosanquet, and Bradley, and Ward, and Stout.) Ormond's grappling with the problem of the antithesis between fact and worth, science and value, is a forerunner of the vital constructions of near-by philosophy, and his attempt to exploit the æsthetic category — a lead which, I think, when fully followed out, will yield the profoundest interpretations of experience — must have its due influence on future thought. (Cf. these things with the harmless dead-level of Professor Ritchie's writings, as for example the title essay of his 'Darwin and Hegel').

That such a distinction of problems, as between the provinces of science and philosophy, is necessary, may appear more clearly when we take the point of view of psycho-physics and consider the development of mind and body together. The postulate of psycho-physics is that whatever is going on at any time in consciousness, something is going on at the same time in the brain. As soon as we come to make this position definite, it becomes clear that we must admit the existence of this two-fold process, both in the factors of experience corresponding to content, and in those corresponding to organization or form. It is equally necessary, for example, that we ask both for the physical process in the case of the sensations which are united in the perception of an object, and also for the physical process of *the function of union itself, which constitutes the object as one.* In other words, there must be a scientific side, an empirical side, to the categories of organization, as well as to the material which is organized in the categories. And to say that certain modes of organization, or laws of thought, are constitutional, and in their original character logical, does not in any way exempt them from this sort of scientific construction. For example, suppose we take the case just cited of what we call 'unity' in mental construction of any sort. It is possible to assume — as the older logical psychology did, and as I suppose Dr. Bosanquet would now be disposed to do — that all thinking involves unity as a constructive principle, and that no mental wholes of any sort are possible except through the use of this category. But it is possible, also, while fully admitting the logical importance of this element in actual constructions, to ask for the genetic processes by which the mind maintains unity at any grade of its construction.[1] Is the unity, let us say, of an individual object accompanied, on the physical side, with some sort of general sensory process by which more partial sensational processes are held together? If so, then on the mental side we recognize in the feeling, or more generally the consciousness, of unity, something going along with the possible separate states of consciousness corresponding to the particular processes of sensation involved. This means that we have, in so far, not only determined the physical process underlying mental unity, but we have also characterized *the consciousness of unity* along with the other elements which go into the perceptual psychosis as a whole. Suppose, on the other hand, we believe, as the present writer believes, that the consciousness of unity is, so far as the physical processes are

[1] Of course I mean not merely de facto unity, but *consciousness* of unity — not merely 'psychological,' but '*psychic*' unity.

concerned, not a sensory process but a motor process. Then we have a different genetic account of the mechanism and possibilities of this category throughout all the constructions of knowledge. On this theory, as I hold it, unity is the mental reflection of motor synergy — the unity of more partial and partially adapted motor processes in a larger whole of adjustment or accommodation. This reflects itself in consciousness as what we call the form of 'unity in variety,' or the 'sameness in difference' of which the philosophers of Dr. Bosanquet's school make so much. It gives us what I believe has never been given before, a legitimate genetic and psycho-physical account of the antithesis between the unity of a mental whole and the variety of its detailed contents. Moreover, just in so far as this unity results[1] in a prospective motor adjustment, by which particular more detailed adaptations become effective in the treatment of experience, just in so far the whole is reflected in consciousness as what we call teleological — as realizing an end. We thus have, from the point of view of science and of the actual processes involved, a genetic account of the teleological wholes which Dr. Bosanquet emphasizes in his criticism of the theory of selective thinking.[2] Apart altogether from the question of adequacy of such an account — a point which cannot be argued in detail in this brief paper — what I should say is that such an account, whatever its detail, in nowise contradicts or interferes with the teleological interpretation of the movement of thought, as Dr. Bosanquet and Dr. Stout depict that movement; and this may be taken, I think, as an example of the sort of supplementary treatment which logical psychology should anticipate and welcome from genetic psychology.

It is astonishing to me, after what I have been accustomed to consider the thoroughgoing reconciliation of logical and genetic points of view in the work of Hegel, that thinkers whose views derive so much inspiration from him should not be able to accept this position. Many of the disciples of Hegel, and indeed also of Aristotle among the moderns, are not as thoroughly genetic as were their masters. There is growing up to-day, however, a new sense of the importance of the problem which is really at the bottom of our differences of opinion here — the problem as to how, if we give naturalistic science its full

[1] I say 'results' because it *is*, as a mental construction or object, a result even *before* active realization — it is a synergy of revived processes.

[2] I do not see how the fact that the details are altered in the new organization affects the validity of the theory, except on the doctrine of extreme atomistic association. The larger whole of motor processes is an essential reorganization, and we should expect the partial contents to be liable to paring-down and reducing changes.

rights, we can still conserve the newer modes and values which arise as the material of science is progressively organized in more advanced and complex stages. How, in short — to modify the famous question of Kant — *how is experience of new reality possible?* — that is, how can experience go forward by successive applications of certain categories and principles, and issue in *essentially novel modes?*

This, I take it, is one of the two burning questions which genetic science is making it possible and most urgent to ask.[1] And I think if we are to have a philosophy of the sort hinted at above, which is based on genetic no less than quantitative science, and includes the formulas of both, we must somehow answer this question before we can proceed very far. Much fear of that bug-bear called 'naturalism' — especially in England — arises here. It proceeds by the definition of naturalism as a view which recognizes only mechanical interaction, and it then tests the right of science to interpret all forms of organization on the basis of such a naturalism. Now, apart altogether from the criticism of this use of the term naturalism — a use of the term in my opinion quite unjustified — we may say that even though such a naturalism carry its interpretations through the whole range of phenomena, nevertheless there still remain other possible interpretations, and among them the genetic, of the same or other aspects of the same phenomena. This is too large a question to discuss in a page, and I have sketched in the recent volume, 'Development and Evolution,' Part III., what is there called the theory of 'genetic modes,' as a tentative line of direction in approaching this problem profitably.[2] If we go to the

[1] The other is that of the interpretation or meaning of the entire genetic series as dealt with by science.

[2] The following quotations may serve to show what I believe to be the correct relation between the two interpretations of fact which are represented respectively by quantitative science, on the one hand, and that sort of science, on the other hand, called genetic, which recognizes the production of new synthetic modes or phases of reality. "How then can there be any such thing as a phase of reality which is not subject to plain statement under natural law? This question represents a very common criticism of all thoroughgoing statements of mental evolution. It rests on the mistaken view that a statement of the historical career of a thing can ever be an adequate statement of its nature; in other words, that the origin of the categories of thought can tell what these categories will do—what their function and meaning is in the general movement of reality. * * * The writer has developed a view of mental development, which not only makes each state of it a matter of legitimate natural history, but goes on to say that the one process of motor adjustment is imitative in type. What could be a more inviting field for the criticism: imitation is mere repetition [so Dr. Bosanquet charges upon my view]; how can anything new come out of imitation? Not only is consciousness merely repeating the relationships

bottom, I think we can 'hold up' nowhere short of the position of certain recent writers (for example, Münsterberg) who hold that the right of naturalism to the treatment of every describable thing, must be admitted, but, at the same time, the equal right of a science which deals with other modes of organization, with meanings and values, over the whole field, is equally to be admitted; and that the problem

already present in nature, but the development of consciousness itself is merely a series of repetitions of its own acts. * * * The counter question may be put: Why cannot anything new come out of imitations? Why may not the very repetition be the new thing, or the condition of it? To deny it is to say that by looking at the former instance, the historical, after its occurrence, you can say that that occurrence fully expresses mental behavior. On the contrary, the prospective reference gained by the imitation may bring out something new; the repetition may be just what is needed to develop an important stage in the career of mental reality. In itself, indeed, an imitation is no more open to the objection we are considering, than any other kind of mental behavior, and it is not allowed that imitation is no more than repetition — though, of course, in certain cases it may be no more. * * * It does not follow that because a product — one of the categories of organization, such as design, the ethical, etc. — is itself a matter of gradual growth, its application to reality is in any way invalidated. * * * The fallacy is just the assumption that reality is finished; that categories of retrospective reference exhaust the case; that the series of events, which are sufficient ground for the origin of the category, might also be sufficient evidence of its validity. That there is a sharp contradiction, therefore, between a doctrine of derivation from experience and application beyond experience" (*Development and Evolution*, pp. 283–6).

"I contend that absolutely new and unheard phases of reality may 'arise and shine' at any moment *in any natural series of events — constituting new genetic modes.* Considering the origin and nature of the categories of thought, whatever our theory of the method of their genesis may be, we find that they are modes of function selected for their utility as furnishing interpretations of experience. It is evident then that it is impossible to discount or deny by their use any modes of existence or reality *which they do not interpret.* * * * The very origin of the categories which we use in science, restricts their application, since there may be other types of experience which are so far untouched and which might be construed only under other categories" (*ibid.*, pp. 305–306).

"We cope with the new by a tentative outreach toward it, armed with our categories of description and historical interpretation. In so far as these are adequate, they reflect earlier stages in the unfolding of the same system. But the 'arming' is inadequate for full interpretation, since the arms are forged in the fires of the past. The ideals, the values yet in process and always to be in process of achievement, have their impelling power from the very experience that knowledge and life are functions of a genetic process of which our formulated realities are passing phases. This might be carried out in a philosophical view of reality — a theoretical doctrine of metaphysics — but that is not my intention here. The only safe course for science, however, is to recognize these things. Genetic science is competent to make the reservation always, in the presence of each of the applications and explanations of exact and numer-

of philosophy — of the metaphysics of the future — is that of finding some final mode of interpretation in which these two world-comprehensive points of view are to be finally reconciled and harmonized.[1]

In the preceding remarks, I have gone a good way afield from Dr. Bosanquet's more detailed criticisms of the theory of selective thinking; but when we come now with these general remarks in mind to take up the points which he makes, I think we shall find that each of them illustrates a phase of this larger issue. For example, on pp. 387–88, he speaks of the teleological aspect of a whole of mental construction as in some way contradicting or invalidating the process of construction given by the theory of selective thinking. I should say, not so. On the contrary, I think that a whole of thought which illustrates and accomplishes a mental end is not only not exempt from the requirement of being psychophysical and phenomenal, but that its only possible realization in experience is through such processes — through traceable genetic processes. We must ask for the genetic side of the constitution of a teleological whole. Furthermore, to cite another instance, he asks: 'Why is material action an excellent test of thoughts?' And his answer is: "Because, in assignable respects, it is a very strong case of thinking with verification, but if you make thought borrow its unity and consistency, having none in itself, from what is found to succeed in action, then I no longer see why we should respect these qualities or treat them as guides to truth or reality. Action no longer reveals a principle that pervades experience." On the contrary, I should say that this completely inverts the requirements of genetic science in favor of a philosophical assumption. If a 'principle that pervades ex-

ical science, that *it is a cross section, not a longitudinal section, to which the quantitative and analytical formulas apply;* or that if they apply throughout a serial process — as in a series of successive transformations of energy — *that* is proof that the process in that case *is not a genetic one.* It is the genetic aspect, we must hold in such cases, which has escaped the formula. The success of the quantitative and analytical method is itself the evidence that no really genetic movement has occurred; in other words, that only those aspects of things have been observed which illustrate the repetitions, not the adaptations, of nature" (*ibid.*, pp. 307–308).

And in stating positively what is called a 'postulate' of the theory of genetic modes, this formula is advanced: "That series of events only is truly genetic which cannot be construed before it has happened, and which cannot be exhausted by reading backwards after it has happened" (*ibid.*, p. 311). But it is maintained that while such a series cannot be *exhausted* by reading backwards, it can, nevertheless, always be *interpreted* by such a process of reading in the categories of exact science.

[1] Personally, I think the æsthetic category goes further in covering the 'multitude of sins' of naturalism than any other!

perience' is not to be revealed through conscious process, how short of a miracle can it be revealed at all? If it is to be revealed, it must be in interpretations or generalizations of actual empirical processes. Now I do not say that thought 'borrows its unit and consistency from action, having none in itself.' This would commit me to a dualism so essentially logical that, *me judice*, science should have none of it. On the contrary, I should say that the unity of 'thought in itself,' *is* the conscious side of the unity or synergy of material actions—that so far from expecting unity and consistency to serve as guides to truth or reality, on the contrary what we mean by truth or reality is just the presence of the unity and consistency of material action. The logical dualism between reality and the thought construction which is supposed to lead to it is mythical. This question is discussed in the address on 'Selective Thinking' and it is also treated with similar results by Simmel in his paper, quoted there. It is one of the results of a genetic theory of thinking that these dualisms between hypostatized realities on the one hand, and mental processes logically divorced from such realities on the other hand, are once for all disposed of. If some mental experience is to 'function as universal,' to use Dr. Bosanquet's own expression, why should it not be a mental experience which has for its physical counterpart the synergy of adapted action? This illustrates the possibility, as said in the broader arguments above, not only of finding genetic processes accompanying the abstract and universal ingredients of our knowledge, but of finding ourselves also compelled to readjust our earlier logical distinctions in view of the results of researches in psychological genesis.

One further word: Why do we need 'thought-in-itself'? Is it not a sort of superstition that 'thought-in-itself' is better than thought in action, or thought in an empirical or genetic series of some kind. Is not to require it really to make a judgment of value, a *Werthurtheil*, to express a preference, and then to refuse to be thoroughgoing in one's science for fear in the end this philosophical judgment of preference may lose its prestige?

In conclusion, I may express the hope that Dr. Bosanquet will give us his further mind on certain of these matters; particularly should I, for one, value his opinion as to the scope of genetic psychology, and also as to the treatment of the successive grades of organization which I have called 'genetic modes.'

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## A SOUND PERIMETER.

Recent studies in auditory space perception have shown that the power to localize sounds rests, to a great extent, upon secondary factors. What unaided introspection would lead us to consider direct acoustic sensory data, exact experiment often reveals to be only associations or the result of subconscious influences of some sort. In future experiments more attention must be paid to the elimination or control of these associations and suggestions. Within the last few years, much good work has been done in the study of the localization of sound, but all with crude and often inadequate apparatus. None of the sound cages, or substitutes for the same, which have been used, could have been operated without giving suggestions that would tend to invalidate the results. Only those who, like the writer, have been engaged in these experiments, can fully appreciate this criticism. Results have been obtained at the expense of wasted time and patience in the effort to conduct the experiments on such plans that the shortcomings of the apparatus might be overcome.

In order to be adequate for most purposes, the apparatus for the producing and registering of the sound which is be to located should permit, among others, the following variations in the stimulus without giving any suggestion or counter-suggestion to the observer: (1) the direction of the stimuli from the middle of the aural axis, (2) the intensity of each of the stimuli, (3) the distance of one stimulus, (4) the number of stimuli to be given simultaneously or in succession, and (5) the order and frequency of stimuli from a given position.

The sound perimeter shown in Fig. 1, has been designed to meet these requirements. It consists of a system of telephone receivers so mounted and connected as to make the above-named variations possible. The main frame is made of iron tubing and braced in such a way as to afford the maximum rigidity with a minimum of material which might reflect sound. The receivers through which the stimuli are produced, are mounted on movable arms, which may be denoted as *A*, *B*, *C*, and *D*, respectively. Arms *A* and *B*, each representing an arc of 135° of a circle whose radius is one meter, are so mounted on a common center at the top that they may swing in the same course, describing a part of the surface of a sphere one meter in radius. Each of these arms carries a pointer, which moves under the circular scale placed above the bearings. This scale is graduated in five-degree units and marked with large figures, which may be read from the experimenter's position behind the tablet on the main sup-

port of the frame. The two arms are mounted on a common axis, but they turn on independent bearings, so that there is no friction between

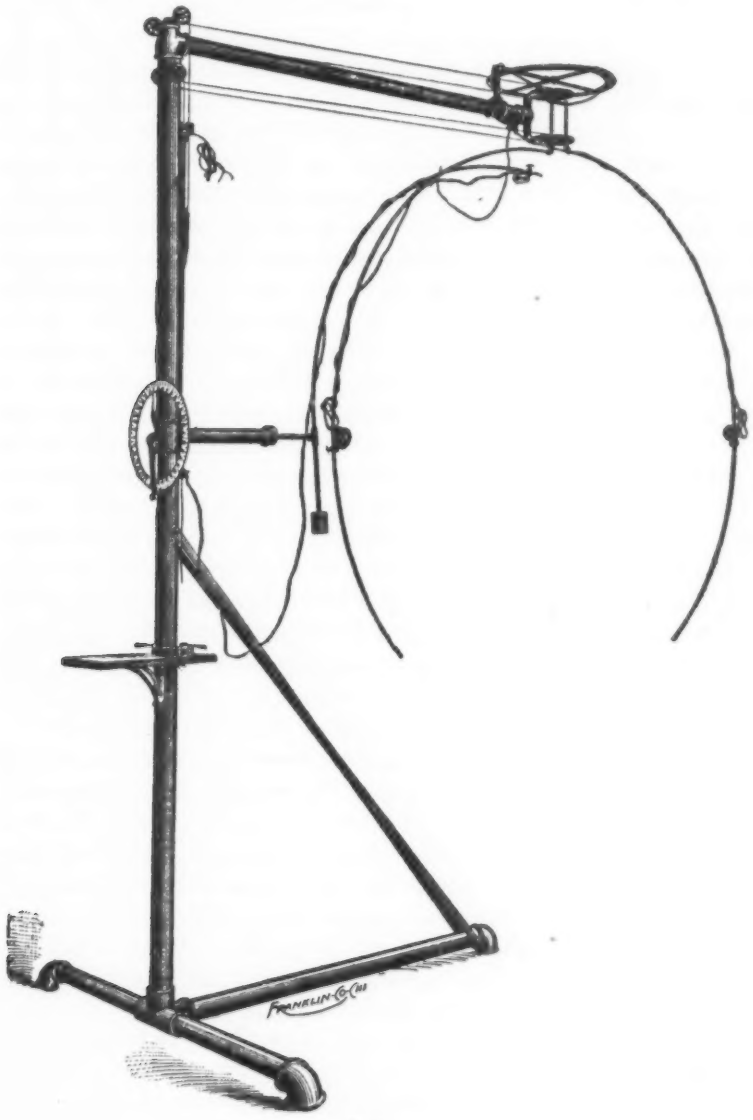

FIG. 1.

them. The arms are turned by means of cords which run from the experimenter's tablet up to pulleys at the top of the frame and thence

to wheels mounted on the upward projections of the arms. There are two of these cords for each arm; pulling one cord turns the arm to the left, and pulling the other turns it in the opposite direction.

The third arm, *C*, turns in the surface of the same sphere as the other two arms, but is mounted on the side and counterbalanced, so that it may be turned readily by means of the crank which is seen directly above the tablet. The pointer on the crank runs over a circular scale which is graduated in five-degree units, in the same manner as the scale for arms *A* and *B*. The axle which carries this arm may be drawn back through the frame so that the arm may pass the other two arms without striking at the top, and so as to be out of the way when not in use.

Arm *C* may be removed by pulling the axle out after detaching the crank, and arm *D*, a straight rod, will fit in its place of support. In Fig. 1, arm *D* is seen only in part, being stored away on the side of the main upright of the frame. This arm carries the receiver on one end, and is graduated in centimeters for guidance in the adjustment of the distance of the receiver from the center of the sphere. The arm is three meters long and slides freely in the horizontal direction.

For ordinary purposes only one receiver is needed on each arm, but it is evident that any number of receivers desired may be mounted on each arm for the purpose of special experiments. The receivers are clamped by a thumb screw and may be placed in any position on the arms. In order to eliminate conduction along the arms, the receivers are insulated from their clamps by means of soft rubber.

Soft and flexible wires run from the receivers to terminals on the surface of the frame and permanent wires are laid from these terminals, inside of the frame, to the tablet. The same circuit is used for arms *C* and *D*, as they are never used simultaneously. There is a knife switch for each circuit on the tablet and all the circuits are completed through the same battery and mercury key (not shown in the cut). Thus, when the key is pressed, a click will be heard in a receiver if the switch in its circuit is closed; and if two or three switches remain closed at the same time, the current is distributed equally to the corresponding receivers and the clicks will occur simultaneously in all.

Resistance may be put in the main circuit or in one or more of the branch circuits, as the needs may be, to vary the intensity of the click. If a dry-battery is used it may be fastened to the frame and then the apparatus will be complete without any further accessories.

To vary the quality of the stimulus, tones of different pitch may be substituted for the click. For that purpose it is necessary to have electric tuning-forks of the desired pitch in a distant room and to complete the perimeter circuit as a shunt through the fork. The tone will then be heard in the receiver whenever the key is pressed.

The center of the sphere described by the arms is 1.73 meters above the floor. A high, adjustable stool is placed under this center and adjusted for the observer so that the center of the observer's head occupies the center of the sphere. If a head rest is used great precautions should be taken to prevent disturbing effects. It is best not to use any head rest, but to check the position of the observer frequently by putting arms *A* and *B* at opposite points and sighting across. The height is determined by reference to the axis of arm *C* or arm *D*.

The scheme for numbering the points on the scale is of considerable importance. That plan has been adopted which students tend to follow spontaneously when asked to describe the location of a point in space. In this there is no number higher than 90. The upper scale gives the reading for horizontal planes and the side scale for vertical planes. The nomenclature adopted may be described without any diagram. The horizontal scale has two zero-points, one in the median plane in front and the other in the median plane behind; *i. e.*, every point in the median plane of the head is at 0° with reference to the horizontal plane of space, and degrees are counted toward the right and toward the left from the median plane both in front and behind. In the vertical scale, the two zero-points are at the level of the ears; *i. e.*, every point in the horizontal plane through the ears is at 0°, and degrees are counted upward and downward from this level. This gives a simple and natural nomenclature for direction, *e. g.*, a point is 'in front, 15° left and 25° up.' The upper scale may be turned so that this system will correspond to any desired position of the observer.

This apparatus will favor the use of the method of right and wrong cases and the method of minimal changes, in which it is not necessary for the observer to estimate degrees. However, it is sometimes advantagous to allow the observer to indicate the direction with a pointer; the experimenter may then swing the perimeter arms to such a point and read off the result on the scales.

This brief statement, supplemented by the figure, may suffice to give a general idea of the apparatus. Its special merits are, that it enables the experimenter to stand in one place throughout complicated

series of experiments and operate all the parts of the apparatus without giving any suggestion by movement or delay, that the movable parts of the apparatus are made to act without sound or jar, and that it makes it possible to vary, measure, and control the essential factors.

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# PSYCHOLOGICAL LITERATURE.

*The Founder of Mormonism.* A Psychological Study of Joseph Smith, Jr. I. WOODBRIDGE RILEY, with an Introduction by Professor G. T. LADD. New York, Dodd, Mead & Co., 1902. 8vo, pp. 446.

The attention of psychologists should be drawn to this study of the originator of a curious and influential religious movement still operative in the United States. The volume is the Doctor's dissertation (Yale) of a careful student, equally conversant with the literary, historical and psychological aspects of the topic. The story is an extremely complicated one, and many of the essential facts are most difficult to discover. As the result of a painstaking search through a great mass of contemporary records, Dr. Riley has succeeded in presenting a consistent—though inevitably in parts a conjectural—reconstruction of the sources and nature of the writings and doings of Joseph Smith, Jr. The keynote of the explanation is a psychological one; that Smith was a neurotic, with distinctive epileptoid symptoms, given to the seeing of visions, steeped in a superstitious form of narrow religious dogma; that his associates and first converts were precisely of that easily suggestible type whose lack of education and religious fanaticism would make them credulous believers of such pretences to revelation and divine authority as Joseph Smith put forth; that the 'Book of Mormon' and many of the revelations that formed the early history of the Church of Latter Day Saints were dictated by the author, Joseph Smith, while the latter was in a quasi-hypnotic condition self-induced — the writings and the account of their production bearing evidences of automatic writing and of the processes observable in 'crystal-gazing.' The force of this argument can be appreciated only in its cumulative mass, and that requires a fair-sized volume to set it forth. The story is confessedly an imperfect one; and further research is not likely to fill in the regrettable gaps in the evidence; still, the internal circumstantial evidence distinctly contributes to the acceptability of the theory which Dr. Riley maintains. Analogical reasoning occupies an important place in the argument, and resemblances to recognized results of the same forms of individual and collective psychoses as those involved in the origin of Mormonism, are

constantly cited. It is somewhat surprising that Dr. Riley's diligent use of recent psychological materials has not brought to his attention the case of the 'revelator' of 'From India to the Planet Mars,' so carefully detailed by Professor Flournoy. In both cases we have the same fantastic explorations of the imagination; the same assumptions of narrative, incidents and style apparently foreign to the subject's normal intelligence; the same invention of fictitious names, persons, places and things; the same possibility of tracing many of the incidents and details of the document to authentic experiences of the subject, but experiences which may have been subconsciously realized and are certainly recorded while in an abnormal state of dissociation; the same periods of incubation preliminary to the further development of a new stage in the automatic revelations; the same participation of the suggestions of others and of the clever adaptation to actual circumstances and incidents, in the subsequent revelations; and so on. The striking differences between the two cases are external and not psychological. In the one case, the revelations are given out as real and inspired, find a congenial soil in which to flourish, and so attain practical significance. In the other case they remain the purely personal expression of a luxurious imagination. Undoubtedly the primitive state of the psychology of abnormal mental states at the time of the miraculous discovery and transcription of the 'golden plates' of the book of Mormon had something to do with the ease with which such a propagandum was disseminated. But we must remember that the kinds of persons who would become converts to such systems are not likely in any community to regulate their beliefs by scientific principles. Recent movements of comparable type in very differently situated communities have revealed the widespread existence of the proper psychological soil for the flourishing of similar delusions. The comparative study of such movements, most of them with a religious tendency, is likely to contribute to their understanding. Dr. Riley's presentation of Mormonism's founder is a notable addition to the collection of psychological documents of this type. J. J.

*Development and Evolution.* JAMES MARK BALDWIN. New York, The Macmillan Company, 1902. Pp. xvi + 395.

In this volume Professor Baldwin undertakes, from a decidedly genetic point of view, the discussion of a series of problems of vital interest both to the psychologist and the biologist. The result is a work in which thinkers in both of these subjects will find much food for reflection and investigation. Though written by a psychologist,

the work may be said to have even greater value for the biologist than for the student of mental phenomena, as disclosing the attitude of recent psychology towards biology and the limitations both inherent in the subject and of the biologist's own making.

The work consists of a number of articles that have already been published in various journals since 1895, worked into a whole with considerable material not hitherto published. All of the chapters bear more or less directly on the subject of organic selection and orthoplasy. For the purpose of making the treatment of these subjects as complete as possible, the various articles of Osborn, Lloyd Morgan, Poulton, Headley and Conn, bearing on the same matter, have been reprinted in a series of appendices. Although this involves a good deal of repetition, as the author admits in the preface, the great advantages of the method are apparent. In these days of scattered publication every psychologist and biologist will be glad to find all of this material in such accessible form. The work thus becomes, as the author claims, a sort of handbook of the theory of orthoplasy.

The work is divided into three parts, which deal respectively with 'The Problem of Genesis,' 'The Method of Evolution,' and 'Criticism and Interpretation.' In the first part we find some valuable definitions of terms, interesting discussions of psychophysical parallelism and of certain conceptions of moment in the comparison of mental and biological phenomena, and a criticism of the theory of preformism from the standpoint of accommodations.

The terms 'development' and 'evolution' are employed in the title and throughout the book in the sense of 'ontogenetic' and 'phylogenetic' development respectively. Mental phenomena as viewed subjectively are called 'psychic,' as viewed objectively 'psychological.' A series of terms in 'nomic' (bionomic, psychonomic, socionomic) are introduced to designate the agencies which condition biological, psychological and sociological phenomena. It is stated that the term 'bionomic' gave the cue to establishing this distinction and the other terms above mentioned.[1]

[1] In a recent article in *Science* the present writer advocated the use of the term 'ethological' in the place of 'bionomic,' although no reference was made to the latter term because he desired to leave the botanists in possession of the term 'œcology,' and because he wished to have a better term for the use of zoologists. The following reasons may be assigned for avoiding the use of the term 'bionomics': First, there is absolutely no reason to suppose that the botanists will use the term 'bionomics' as the term 'œcology' is thoroughly established in their science. Second, while 'taxonomy' and 'taxonomics' mean the same thing, 'bionomy' and 'bionomics' do not, at least according to the

In his discussion of psychophysical parallelism Professor Baldwin recognizes two leading principles: "First, the principle of *equal continuity*, to the effect that there can be no breaks in either series of changes, the brain changes or the conscious changes, without a corresponding break in the other; in other words, if one of the series be continuous, the other must be continuous also." "Second, the principle of *uniformity*, to the effect that the sort of modifications which are associated one with another in brain and mind are always the same; that is, if a certain brain process be correctly hit upon as essentially associated with a certain conscious state, then the concomitance of these two terms may be looked for on all other occasions of the occurrence of either of them. This formulation is, it is easy to see, absolutely necessary to any science of psychophysics at all." A discussion of the theories of heredity is adroitly avoided and is, indeed, quite unnecessary to the argument. "The question of the 'continuity of germ plasm' may be decided one way or the other — either for or against the actual transmission of an identical substance — without raising the question of a corresponding transmission of anything psychological. For if there be breaks in the psychological series at those nodal points at which generation succeeds generation, there are also, by the principle of equal continuity, discussed above, breaks in the psychophysical, and we may find the psychological series beginning again at the appropriate point in the development of the organism of the new generatian — the point at which the psychophysical again begins. In other words, the advantage gained from the psychophysical point of view is that if there be no gaps in one of the series,

dictionaries, and there is a chance for confusion here. Third, 'bionomics,' in the sense of 'ethology,' is hardly used outside of England, and even in that country it does not seem to be in very general favor. Thus Dr. Bather and Dr. Sharp inform the writer that the Royal Society recommends the use of 'ethology' as the proper heading for works on habits, etc., in the 'International Catalogue of Scientific Literature,' and the latter gentleman calls attention to the use of this term for some years past in the 'Zoological Record,' and expresses the opinion that it will be generally adopted. Fourth, 'bionomics,' in the sense in which it is used by Professor Baldwin, emphasizes the environmental factor in adaptations, whereas 'ethology' emphasizes the side of the organism, the 'true set of forces whose play is conditioned' by the environment. This is no unimportant distinction in these times of piping physical and chemical dogmatism in biological science. Fifth, the uniform use of 'nomic,' as suggested by Professor Baldwin, breaks down in the well-established term 'taxonomic,' which does not refer to anything conditioning something else (unless we distort Professor Baldwin's meaning in this case to imply that the structures of organisms condition their position in the classification) but is used in the sense of the word *νομος* in 'bionomy.'

we may either assume them filled up by theoretical parallelism with the other series at these points, at which it has no gaps, or we may — if we deny continuity to either — make gaps in the second series in correspondence with the gaps found in the first. We have in any case, in short, either a psychophysical fact, or we have not: if we have, then either series is sufficient to carry us over the critical point; if we have not, then the break in one series is sufficient evidence of a corresponding break in the other also. The principle of parallelism assumed, we claim once for all the right *to neglect the relation of the two terms, mental and physical, in all circumstances whatsoever.*" Thus the same set of formulas will cover both sets of facts, and we are at liberty to use "the mental facts as antecedents of the physical facts, often the physical as antecedent of the mental, and again, often the psychophysical as antecedent of either or both."

The second chapter, on 'Comparative Conceptions,' shows that Professor Baldwin has none of the squeamishness of many psychologists about adopting a thoroughgoing genetic point of view in psychology as well as biology. In previous works (notably in his 'Mental Development of the Child and the Race') he had extended the principle of natural selection to the selective adjustments effected by consciousness from over-produced functions (movements, dispositions, etc.) on the basis of Spencer and Bain's theory of 'surplus discharges.' In the present work this principle finds further application and emphasis. "A completed view of the psychophysical accommodation requires (first) natural selection, operating upon (second) variations in the direction of plasticity, which allows (third) selective adjustment through the further operation of natural selection upon the organism's functions."

The section on 'Correlation of Characters' is of considerable interest, although dealing with a notoriously obscure and refractory set of phenomena. The important conclusion is reached that "the psychophysical question, when put in a particular case, is really one of determining the correlation of the characters which consciousness shows with those of the organism as such. And it follows that, wherever a mental character enters into the complete carrying out of a physical function, it must have its place assigned to it." * * * "Furthermore, we may say that no physical character which has mental correlations is completely understood until these latter are exhaustively determined, and also that no mental character escapes physical correlation. Recent researches in the psychological and physiological laboratories is establishing many such psychophysical correlations: that of emotion

with motor processes, of attention, rhythm, and the time-sense with vaso-motor changes, that of mental work with nervous fatigue, and so forth through all the main problems of this department. All this affords, in so far, at once illustration and proof of the general formula of psychophysical parallelism." According to Professor Baldwin, it is only recently that the idea of selection from congenital variations has been brought over from biology into psychology. Here, too, is seen an instance of the close correlation of the mental and physical. "Natural selection has worked upon correlated psychophysical variations — not upon organic variations merely. In other words, it has been the psychophysical, not the physical alone, nor the mental alone, which has been the unit of selection in the main trend of evolution, and nature has done what we are now urging the science of evolution to do — she has carried forward the two series together, thus producing a single genetic movement. It would have been impossible for mind to develop by selection with reference to utilities for which the necessary organic variations were not present; and so also it would have been impossible for the organism to evolve in ways which the consciousness of the same animal forms did not support and further. There could not be independence; there must be correlation." The fact of correlated variation is also applicable to organic and mental variations in different individuals. As an excellent example of this, Professor Baldwin calls attention to the theory of sexual selection, in which a correlation is assumed between the organic ornamentation of the male on the one hand and the mental apprehension and sexual impulse of the female on the other. The very interesting results of Groos's study of play in animals are also utilized in this connection.

In the third chapter the reader's attention is directed to the two leading principles to which the work is devoted. These are best stated in the author's own words, even at the risk of overdoing the number of quotations in this review. These quotations should be prefaced by the remark that Professor Baldwin, like some other recent authors on evolution, understands by 'modifications' the 'somatogenic variations' of biologists, and by variations proper the 'blastogenic' or 'germinal variations.' The first principle is that of organic selection. "This position is the general one that it is the individual accommodations which set the direction of evolution, that is, determine it; for if we grant that all mature characters are the result of hereditary impulse plus accommodation, then only those forms can live in which *congenital variation is in some way either 'coincident' with or correlated with the individual accommodations which serve*

*to bring the creatures to maturity.* Variations which aid the creatures in their struggle for existence will, when definite congenital endowment is of utility, be taken up by the accommodation process, and thus accumulated to the perfection of certain characters and functions. The evolution of plasticity, on the other hand, could only itself have taken place by the coöperation of accommodation using the variations towards plasticity already present at each stage, and thus saving and developing such variations. This gave an even higher platform of variation from which steady refinement of plasticity and its accompanying intelligence was all along possible. *Organic selection becomes, accordingly, a universal principle, provided and in so far as, accommodation is universal.*

"Accommodation, therefore, when all is said, is a positive thing, a vital and mental functional process supplementary to the hereditary impulse. It must be considered a positive factor in evolution, a real force emphasizing that which renders an organism fit; whereas natural selection, while a necessary condition, is yet a negative factor, a statement that the most fit are those which survive. If it be true that those variations which can accommodate, either very much or very little, to critical conditions of life are the ones to survive, and that such variations will be accumulated and will in turn progressively support better accommodations, then it is the accommodations which set the pace, lay out the direction, and prophesy the actual course of evolution. This meets the view of the Lamarckians that evolution does somehow reflect individual progress; but it meets it without adopting the principle of Lamarckian inheritance.

"The second general position advocated, on the basis of facts, in the following pages is that of Social Heredity or Social Transmission with Tradition. This too falls into place in our general theory of determination. If accommodation is a fact of real and vital importance, then some natural way of regulating, abbreviating and facilitating it would be of the utmost utility. If animals were left to constant experimentation each for himself, they would die, as we have said above, before they made much development. We find that an important function of consciousness is that it enables them to profit by experience. By memory, association of ideas, pleasure and pain motivation, they abbreviate, select and handle experience to the most profit.

"But there also arises an additional resource — and certainly a very important one — by which *they are enabled to profit as well by the experience of others.* So soon as animals can use their native

impulses in an imitative way, they begin to learn directly, by what may be called 'cross-cuts' to a desirable goal, the traditional habits of their species. The chick which imitates the hen in drinking does not have to wait for a happy accident, nor to make a series of experiments, to find out that water is to be drunk. The bird deprived of the presence of others of its kind does not learn to perfection its proper song. All the remarkable accommodations of an imitative sort, so conspicuous in the higher animals, enable them to acquire the habits and behavior of their kind without running the risks of trial and error. Calling this store of habits of whatever kind 'tradition,' and calling the individual's absorption of them and his consequent education in tradition his 'social heredity,' we have a more or less independent determining factor in evolution. For these accommodations are the cream of the needs of life, they represent the essentials of education, the *sine qua non* in an animal's equipment; so the accommodations which must be reproduced in race evolution, as adaptations which the species must effect, are in these lines. The influence of organic selection is, therefore, exerted to determine, by the selection and accumulation of variations, the congenital equipment which most readily utilizes and supplements these traditional modes of behavior. The two factors work together and for the same general result.

"There is, therefore, in tradition a further determining factor. Natural selection plays about it to fix a requisite function here, to eradicate what is unnecessary and nonuseful there — in short, by its omnipresent operation on this character and on that, to perfect the individual for the most adapted life."

In concluding the first part of the work Professor Baldwin stops to advance a serious objection to the theory of preformism and at the same time to answer one of the objections that have been made to the principle of organic selection. It has been claimed by the preformists that the accommodations of the organism are sufficiently accounted for by the natural selection of the congenital variations that make for plasticity. There is, therefore, no reason to emphasize the modifications themselves. The author meets this objection by showing that it is the actual development which dominates and determines evolution, not the variations by which certain functions are merely rendered possible. "Let us say, for instance, that the female bird has a certain capacity for preferential choice among possible males. This means nothing unless she actually makes a choice. Then the physical characters of the offspring vary according as this male or that is

chosen, and these go down to posterity. It is the result which is the evolution, and it is conditioned upon the use made of the endowment."

With the exception of its last chapter, the second part of the work, on the 'Method of Evolution,' consists of a collection of articles that have been published in *Science*, the *American Naturalist* and the author's 'Dictionary of Philosophy and Psychology.' This is the most valuable portion of the book, as it teems with interesting suggestions and sound comments on current biological theories. The first chapter is devoted to a criticism of the late Professor Cope's well-known article in the *Monist* (July 26, 1895), and points out some of the fallacies of Lamarckism. The following passage anticipates the consideration of instinct to which the two succeeding chapters are devoted: "The only apparent hindrance to the child's learning everything that his life in society requires would be just the thing that the advocates of Lamarckism argue for — the inheritance of acquired characters. For such inheritance would tend so to bind up the child's nervous substance in fixed forms that he would have less or possibly no plastic substance left to learn anything with. Such fixity occurs in the animals in which instinct is largely developed; they have little power to learn anything new, just beause their nervous systems are not in the mobile condition represented by high consciousness. They have instinct and little else."

Professor Baldwin's discussion of instinct from the standpoint of organic selection is particularly valuable to the comparative psychologist. With the inheritance of acquired characters the origin of instinct from 'lapsed intelligence' is also of necessity repudiated. Nevertheless the author recognizes the basic value of intelligence, which he uses in a very liberal sense as the equivalent of consciousness — but shows that this very assumption enables us to dispense with the Lamarckian factor. Intelligence "is itself a congenital variation; but it is also the great agent of the individual's accommodations both to the physical and to the social environment." And he remarks further on by way of summary: "1. We reach a point of view which gives to organic evolution a sort of intelligent direction after all; for of all the variations tending in the direction of an instinct, but inadequate to its complete performance, *only those will be supplemented and kept alive which the intelligence ratifies and uses for the animal's individual accommodations.* The principle of selection applies strictly to the others or to some of them. So natural selection eliminates the others; and the *future development of instinct must, at each stage*

*of a species' evolution, be in the directions thus ratified by intelligence.* So also with imitation. Only those imitative actions of a creature which are useful to him will survive in the species; for in so far as he imitates actions which are injurious, he will aid natural selection in killing himself off. So intelligence, and the imitation which copies it, will set the direction of the development of the complex instincts even on the Darwinian theory; and in this sense we may say that consciousness is a factor without resorting to the vague postulates of 'self-adaptation,' 'growth-force,' 'will-effort,' etc., which have become so common of late among the advocates of the new vitalism.

"2. The same considerations may give the reason in part that instincts are so often coterminous with the limits of species. Similar creatures find similar uses for their intelligence, and they also find the same imitative actions to be to their advantage. So the interaction of these conscious factors with natural selection brings it about that the structural definition which characterizes species, and the functional definition which characterizes instinct, largely keep to the same lines."

It is the opinion of the present writer that however valuable the questions of imitation and social heredity may be in the case of man and the higher animals, their unguarded introduction into a general discussion of instinct may lead to some confusion, for it is evident that the vast majority of the lower animals, *e. g.*, hundreds of thousands of species of insects, each with its own more or less sharply defined instincts, are neither gregarious nor social, nor ever even see their parents nor witness the activities of other members of their own species. Hence there can be absolutely no imitation or social heredity in the development of these instincts which are often, nevertheless, extremely complicated (*e. g.*, in the ant-lions, yucca moths, solitary bees, wasps, etc.). In these cases, as Professor Baldwin would probably admit, we must rely on natural selection — or to put it in his terms — the absence of imitation would permit the full incidence of natural selection, *i. e.*, imitation could not act as a screen to protect the organism while it is acquiring the congenital basis of instinct.

Space will hardly permit of an adequate consideration of Professor Baldwin's views on orthoplasy, which is another name for the principle of organic selection involving the repudiation of Lamarckism and the thoroughgoing appreciation of the interaction of modifications and natural selection in producing determinate evolution. This theory certainly holds out the hope of being able to pilot us safely between the Scylla of inherited somatic characters and the Charybdis of imaginary germinal architectonics. It is, moreover, perfectly logical in con-

struction and therefore of considerable value as a hypothetical explanation. Its heuristic value is another matter, and it is here that the biologist will probably demand further light from Professor Baldwin and the other supporters of the principle. Apart from the difficulty of distinguishing very satisfactorily between modifications and variations, it is not at all easy to see how we are to go about testing the hypothesis of organic selection by observation or experiment. This is, of course, no argument against the truth of the hypothesis, but it is nevertheless a serious defect and one which it shares with nearly all the various selection hypotheses. There is also considerable doubt concerning the universality of organic selection, especially in cases of 'passive' characters, like coloration, sculpturing, pilosity, etc., which are nevertheless the very things that the biologist who is trying to work from the relatively solid basis of specific variations, desires most to explain. This seems to be admitted by Professor Baldwin when he says: "Mimicry and those anatomical and structural characters in which the element of function is much reduced, seem to be explained by natural selection with little supplementing from other factors." Still, it may be asked: Why may not natural selection suffice also in other cases of reduced function, especially when we consider the marvellous perfection of adaptation in mimicry and passive structures and the occurrence of determinate evolution in these cases also? A similar question may be asked in regard to the highly perfected instincts of the non-social insects alluded to above.

In his consideration of the objections that have been urged against organic selection Professor Baldwin expresses the belief that the principle should not be restricted to coincident variations but include correlated variations as well. This is supposed to meet the objections of Plate ('Ueber Bedeutung und Tragweite des Darwin'schen Selectionsprincips,' 1901,) which appear to be known to the author only from the report in the *Année Biologique* for 1901. The gist of Plate's argument that 'the blastogenic variations are too small in comparison with the functional adaptability of the organism to have selective value' does not seem to be squarely met by Professor Baldwin, and, if the present writer correctly grasps his meaning, he considerably weakens, at this point in his work, the value of his principle from the standpoint of determinate evolution by too much stress on the obscure possibilities of correlative variation.

The third part of the volume contains considerable new material, embodied in the chapters on the 'Struggle for Existence and Rivalry,' 'Lamarckian Heredity and Teleology' and the 'Theory of Genetic

Modes.' All of these chapters, and especially the last, have a decided bearing on current tendencies in biological science, and will be read with approval by those who are able to appreciate the exaggerations and limitations of the neovitalistic and biometric movements and the current physico-chemical dogmatism in physiology. The last chapter also contains an able criticism of Pearson's views on the subject of history and of Prof. T. H. Morgan's somewhat vacillating opinions on organic regeneration. The chapters on 'Selective Thinking' and 'The Origin of a "Thing" and its "Nature"' are more abstruse and involve questions of more especial interest to the psychologist.

Although the nature of the subjects treated in the volume necessitates careful reading, and sometimes frequent reperusal, no biologist nor psychologist who reads the work will be able to lay it down without a feeling of gratitude to Professor Baldwin for the many helps and suggestions which it contains. Greater familiarity with the details of biology would perhaps have enabled the author to enliven the articles with more richness of illustration and have thereby extended the good influence of the work to that greater body of biologists who fail to appreciate the extent to which psychology is destined to illumine many of the dark recesses of their science.

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## ANTHROPOLOGICAL.

*Reports of the Cambridge Anthropological Expedition to Torres Straits.* Vol. II., 'Physiology and Psychology.' Part I., 'Introduction and Vision.' W. H. R. RIVERS. With an Appendix by C. G. SELIGMANN. Cambridge, The University Press, 1901. 4to, pp. 140.

*The Color Vision of the Natives of Upper Egypt.* W. H. R. RIVERS. Journal of the Anthropological Institute, Vol. XXXI., July–December, 1901, pp. 229–245.

Although the 'anthropology' of the philosophers has usually been equivalent to psychology (in a broad and somewhat obsolescent sense of the latter term), it will not be disputed that the modern science of anthropology has chiefly embraced studies in the physical structure and development of man, in the history and distribution of races, and in comparative culture and linguistics, together with a certain amount of archæology. Logically, however, the comparative study of the mental processes of peoples forms as legitimate a branch of anthropology as the study of their mental products, and in point of fact a re-

newed interest in the psychological problems of comparative anthropology has recently stimulated systematic attempts to collect trustworthy data available for exact inductive interpretation. Field work in this direction has scarcely begun, but the time is ripe and the opportunity fast fading away under the glare of spreading civilization. Experimental psychology has evolved methods and devised tests, which, if not calculated to discover the innermost secrets of the soul, are nevertheless available for extensive application in the field, and adequate to the definite settlement of numerous important questions.

Psychologists, in congratulating their industrious fellow-workers, the anthropologists, upon adding another side to their already many-sided labors, cannot but congratulate themselves as well upon the promise with which such investigation is fraught for psychology.

Dr. A. C. Haddon has rendered a signal service to two related sciences by the excellent provision made for the psychological examination of a primitive people — the Papuans are among the 'lowest' of the races — in connection with the Cambridge Anthropological Expedition to Torres Straits in 1898. It is intended to publish a series of Reports in six volumes, the second to be devoted to 'Physiology and Psychology.' Of this volume, Part I., by Dr. W. H. R. Rivers, has appeared. The 'Introduction' (pp. 1–7) defines the work as 'an attempt to study the mental characteristics of the natives of Torres Straits and the Fly River district of British New Guinea by the methods of experimental psychology.' The chief part of the work was done on Murray Island, where a disused missionary's house was fitted up as a laboratory. Dr. Rivers was here assisted by Messrs. W. McDougall and C. S. Myers, and elsewhere by Mr. C. G. Seligmann.

The difficulties to be encountered in thus dealing with a primitive people have been abundantly recognized by Dr. Rivers, who was able to overcome many of them, as his account of the circumstances shows. Appeals to the vanity of the natives proved especially efficacious in enlisting their interest. The smallness of the mean variations in most of the quantitative records and the general consistency of the results[1] tend to confirm the favorable impression of the trustworthiness of the observations in the minds of the experimenters. The tests were carried on at intervals through several months, many being repeated upon the same individual. The records are usually in the form of averages with mean variations — these determinations for both the individual and the group. In some cases the median is also given. The bear-

[1] Attention is called to the fact that the procedure was with these natives ideally '*unwissentlich*' in almost all cases.

ing of age and sex was considered as far as possible, although it was not always possible to learn the age.

In the following account of the results on vision no attempt at complete summarization can be made of a monograph which is itself a summary of a varied series of tests; besides, the inconclusiveness of many of these renders the records unfit for quotation apart from the accompanying discussions and qualifications in detail. While many of the results are extremely suggestive—even tantalizingly so—most of them require verification by further experiments. At this juncture it should be remarked that as a contribution to the empirical study of methods there can be no doubt of the value of this research. Extreme care is given throughout to the consideration of sources of error which are recognized when not eliminated. Historical summaries of the previous work in the several departments facilitate interpretation of the new facts. I can only quote a few of the more striking results and indicate the upshot of our author's conclusions.

The results fall under four heads: I., 'Physical Characters and Diseases of the Eyes' (pp. 8–11); II., 'Visual Acuity' (12–47); III., 'Color Vision' (48–96); IV., 'Visual Spatial Perception' (97–132). An Appendix, by Mr. Seligmann, deals with 'The Vision of Natives of New Guinea' (133–140).

The almost universal pigmentation of the conjunctiva, the prevalence of opacities of the cornea, of pterygium and pinguicula, the smallness of the pupils in general, are the main points under I. The second and third sections concern us more; the fourth might concern us most of all if only it led us anywhither unmistakably.

Snellen's E-test gave the most satisfactory results on visual acuity, though the ordinary letter test-types of Snellen, Snellen's counting test (No. LIV.) and Guillery's test (visibility of black dots on white ground) were used. The tests were made in the open air, and ordinarily with both eyes open. Beginning at a distance of fifteen meters from the type, the subject was asked to hold a board stamped with the letter E in such a position that the E should resemble the position of the letter indicated on the chart. The distance at which letter No. 5 [1] was read correctly nine times in ten gave the measure desired. Learning by heart seems to have been ruled out in these tests. The reliability of the method is evidenced by the agreement of results obtained on different days, which shows that differences in illumination were not a significant factor.

[1] *I. e.*, the letter which subtends an angle of five minutes at a distance of five meters.

In general, 'visual efficiency' (*Sehleistung*) rather than 'visual acuity' (*Sehschärfe*) in Cohn's restricted sense was measured by these tests, but the latter term is here, as commonly, used to cover both functions.

The average determination for the 169 individuals tested in the Torres Straits and the Fly River region taken together, was 10.6/5, with a median of 11/5. This shows a superiority to that of the Heligolanders tested by Cohn, the only results comparable to these, the averages being 2.12 and 1.77 respectively. We can hardly speak of the average acuity of civilized races, seeing that tests have usually been made upon special classes or by means and under conditions which render them incomparable with the present series.[1] The facts, 'so far as they go,' seem to show that in a state of nature there is no marked sexual difference in visual acuity — but they go a very little way. In Torres Straits 'a very distinct and progressive decrease of visual acuity from the age of thirty-five onwards' is recorded, which is partly attributable to pathological changes less common among Europeans, whose acuity is said to decline at about the age of fifty. While a slight hypermetropia is considered normal for savages as for children, and some facts suggest this condition among the Murray Islanders, Dr. Rivers' results were not decisive. Myopia was extremely rare — a circumstance which would tend to raise the average visual efficiency. "Several cases of astigmatism were discovered in Murray Island." A few experiments suggest that the Papuan eye adjusts itself to the dark more quickly than that of a European. The just perceptible difference of brightness was measured by the use of Masson's discs. While the threshold was higher than that previously recorded for Europeans, results quite as good were obtained, by the same method and with the same discs, from Englishmen, after Dr. Rivers' return. There is thus no evidence of a superior sensitiveness to brightness on the part of these savages.

The conclusion of the whole matter of comparative visual acuity in primitive and civilized peoples (assuming the doubtful right thus to lump diverse racial groups)— so far at least as this research goes— is that, while there is an average superiority in the savage's favor, this is, after all, but slight, especially when the cases of myopia, etc., are excluded, so slight indeed as to render the common report of travelers untrustworthy or as to require that the traditional interpretation be altered. Practice and special knowledge of familiar objects are the two principal factors suggested, in support of both of which Ranke's

[1] The standard $V = 1$ is of course subnormal.

interesting observations are cited.[1] The apperceptive factor, illustrations of which are familiar, invites further study, both qualitative and quantitative.

The special richness of their vocabulary, no less than other obvious signs of familiarity with natural objects, proves the natives to be close observers of nature. In this connection, Dr. Rivers offers a highly interesting suggestion which invites searching analysis and inductive investigation, and which, if true, might prove to have important practical bearings. According to this suggestion, the predominance of minute attention to objects of sense hinders higher mental development. Although the mind must be fed of the senses, it may be overfed and starved through its very gluttony! "If too much energy is expended on the sensory foundations, it is natural that the intellectual superstructure should suffer." (Let our systematic naturalists take warning!) Ranke's unfortunate experience in losing his power to enjoy scenery in the large after living with South American natives and acquiring their habits of particularized attention, together with his still more unfortunate failure in the effort 'to devote attention to the more serious problems of life,' is cited in support of a causal connection between the alleged sensory plethora of the savage's mind and his lack of any considerable æsthetic appreciation of nature.[2]

[1] *Correspondenz-blatt. d. deutsch. Gesellsch. f. Anthrop.*, S. 113, 1897.

This opens a field for fruitful experimentation. One gathers from Dr. Rivers' discussion that while the average acuity of the savage is naturally but little higher than that of his civilized brother, by the constant practice which his mode of life affords he may acquire a superior acuity. Whether this is so in general, or whether practice improves only the ability to descry certain special objects, would be an interesting problem for study. Ranke evidently meant to assert increased general acuity by practice in accommodation for distant objects.

[2] The general question of the relation of the data of perception to the higher mental syntheses is thus revived and set in a new light. Mr. Galton's results on mental imagery in relation to maturity and intellectual occupation with abstract thought are suggestive in this connection. The economy and increased scope of symbolic thinking render it advantageous in evolution, while it implies a correlative neglect of sensory minutiæ. There is some reason, however, to doubt the alleged contrast between the typical savage mind, immersed in a sensuous medium by which it is pressed if not oppressed from every side, and the free mind of your *fin de siècle* philistine, soaring in the dry atmosphere of abstract thought high above the diversified surface of things, and contemplating the whole as might a bird possessed of the requisite apperceptive faculty. Not that Dr. Rivers asserts such a contrast baldly — indeed many of his conclusions counteract, if they do not nullify, this doctrine. But it seems worth while, in view of the suggestion under review, to bear in mind how insidiously, yet how completely withal, we may be deceived by appearances in inter-

Perhaps the section on 'Color Vision' comprises the most interesting results of the investigation.[1] Results of actual tests of color vision were compared with the color nomenclature. The bearing of this on the one-time active theory of Gladstone, Geiger, Magnus, *et al.* is such as to breathe into it a new life and so to give it a 'fighting chance' again.[2]

Holmgren's wools, supplemented by others, were matched by over two hundred individuals. The descriptions of the tests should be read in full for their suggestiveness on many points of method and of theory. Transient comparisons were noted as well as definite decisions. Red, pink and yellow were sorted normally, but green was frequently compared with blue and blue with violet. The tendency[3] to match differently sensed colors having a common name in a limited color vocabulary, especially with the paler 'cold' colors, was noted, along with other sources of error more easily to be guarded against. The tendency to match saturations rather than tones, depending in part upon the linguistic factor, introduced another complication.

While there may have been yellow-blue blindness in a few cases, the facts do not compel such a conclusion. Among 152 individuals tested, including 130 males 'with the just possible exception' of one man, 'there was not a single case of red-green blindness.' The ab-

preting the minds of peoples having a mental tradition wholly different from our own, correlated it may be with a markedly different environment. (Professors Franz Boas and Livingston Farrand are inclined to the extreme view of an essential uniformity of psychic constitution and even of psychic capacity as between the so-called 'higher' and 'lower' races. See some weighty considerations relative to this point in a paper by Professor Boas on 'The Mind of Primitive Man,' in *Science*, Vol. XIII., No. 321, February 22, 1901, pp. 281 ff.) This perceptual enslavement to the particular objects of immediate utility in the daily routine, on the part of a people without the intellectual leisure which comes of a specialized civilization and a comminuted industry requiring little attention to details in general on the part of each, but a limited attention to specific details only, and without the necessity of dealing with the larger relations implied by such a civilization and the concomitant competition of wits, may possibly serve to differentiate mentally the 'primitive' from the more 'advanced' type. But of course no one will pretend that persistent inability to see the woods for the trees is a defect confined to savage peoples.

[1] See an excellent account of these and other results in relation to the whole question of 'Primitive Color Vision' by Dr. Rivers in *Pop. Sci. Monthly*, LIX., May, 1901, pp. 44–58.

[2] Many of the criticisms of Virchow and others will of course continue to hold against the theory, which must ever appear ludicrously weak in its Gladstonian form.

[3] Mr. Seligmann notes the absence of any such marked tendency in British New Guinea.

sence of Daltonism in this race is further supported by results obtained with Lovibond's tintometer. Comparative tables of the threshold values for saturations of red, yellow and blue (when correctly perceived four times in five) reveal some interesting differences between the Murray Islanders and Englishmen. To the Murray Islanders red is most easily recognized, next yellow and finally blue (averages for 17 natives: R =17.6, M. V. 7.66; Y = 26.5, M. V. 9.71; B = 60.0, M. V. 16.5). Yellow takes precedence with the English, while red and blue differ relatively little (averages for 18 persons: R = 31.7, M. V. 22.5; Y = 20.5, M. V. 8.11; B = 36.4, M. V. 15.13). The greater variability of the English arises chiefly from cases of red-weakness. No indication of color-blindness was given with the tintometer, nor even any insensitiveness to red.

It should be remarked, however, that among eight Lifuans (of the Loyalty Islands), three were distinctly red-green blind. There are other considerations which reveal the utter futility of all attempts to generalize the color vision of 'savages' *vs.* 'Europeans.' Dr. Rivers suggests that the prevalence of color blindness may prove to be an ethnic character of taxonomic significance.[1]

In the absence of a perimeter, quantitative observations of indirect color vision were not made, but, by means of rough tests made by moving black cards with colored patches, several interesting features were brought to light. There was, *e. g.*, little difference between the red, yellow and blue fields, although the blue tended to be largest. The green field was relatively small. The color fields seemed generally smaller than in Europeans. The ready recognition of blue was not only conspicuous but in conflict with the results of the other tests and the defective nomenclature to be noted. This discrepancy is attributed to the influence of the pigmentation of the *macula* in direct vision. The large size of the red field and the high sensitiveness for red among the savages seem at first blush to bear adversely upon the atavistic theory of color-blindness.

Contrast (with a color wheel and Meyer's experiment) was apparently less vividly perceived by these natives than by Europeans. Tests for color preference proved unsatisfactory. The probable influence of factors summarized by the word 'tradition' seems to me to invalidate conclusions drawn from Sunday costumes, in which black clearly predominated with the men, while red seemed to take the lead among the women.

Not least interesting are the observations collected upon the color

[1] Cf. the case of the Egyptians below.

vocabularies, obtained by requiring Rothe's papers, various wools and other colored objects to be named. The formation of color adjectives by reduplication from the names of natural objects is very interesting. Thus three words used for green in Murray Island emphasize bile, leaf and pus respectively. The relationship between words for different shades of the same color, and other linguistic features, cannot be discussed here, but only some of the general results of psychological interest. Three color vocabularies of the Torres Straits and the Fly River district agree in having relatively definite and well established words for red and yellow. All three agree in having each a definite word for green, which is not used so unanimously, however, as the words for red and yellow. The Kiwaians have no word for blue; the Murray Islanders, lacking a native word, have modified and reduplicated the English form; in the Western Tribe of Torres Straits a native word occurs, which may be used for green as well. Australian tribes were also examined to some extent, with the result that a still more primitive condition was discovered, in which there were definite names for only red, black and white.

In general there was found among all these tribes an undifferentiated vocabulary for blues and the adjacent hues, which was loosely applied. (Blue sky, green sea, dirty water and black night were denoted by common names.) *What is more to the point, there was an undifferentiated color sense to correspond, as shown by the wool tests, tintometer, etc.* This correspondence lends some justification to the fundamental assumption of the Gladstone-Geiger school. It must not be supposed, however, that the correspondence was at all exact. The vocabularies probably fail to express all the differences actually perceived. Indeed the tribe having 'the most incomplete and indefinite color vocabulary met with in British New Guinea' — the Sinaugolo — was 'specially quick and correct' in matching wools (Appendix, p. 135).

There can be little doubt, thinks Dr. Rivers, that blue is a darker or duller color to the Papuans than to us. But he admits that physiological insensitiveness is only one factor in determining the defective nomenclature, and cites evidence corroborative of the theory which insists on the relation of the distribution of natural pigments to the rise of a color nomenclature, while suggesting other factors, among which may be mentioned the alleged absence of æsthetic interest in nature, with her beauty of earth and sea and sky.

Visual space perception was examined in various ways. Binocular vision was tested with a form of Hering's 'fall experiment,' in which

shot varying in size was dropped before or beyond a fixation point consisting of a bead suspended in a cardboard tube at a distance of two feet from the observer's eyes. Sixteen individuals gave definite results demonstrating ordinary binocular vision. They were right 'nearly every time' with two eyes, while 'as often wrong as right' with one eye.

Papuans saw double images; coördinate eye-movements were observed in a single Papuan baby, aged twelve hours; etc.

The results obtained from dividing lines and estimating lengths and from the illusion tests do not lend themselves to summarization, nor can much importance be attached to them. Dr. Rivers' discussion is, however, instructive throughout and his meager results should be provocative of renewed and more extended investigation. While certain constant errors appeared which differed quantitatively from those obtained with English students or were of opposite nature, it is of interest that these differences tend to disappear when comparison is made between Papuan and English *children*. This comes out, *e. g.*, in the overestimation of vertical as compared with horizontal lines, an illusion which has probably been tried by Dr. Rivers on a larger number of individuals than by any one else. The Murray Islanders are more susceptible to this illusion than English students. More interesting perhaps is the result that by all groups (twenty Papuan men, twelve boys; fifteen English students, twelve children) the vertical was made shorter when drawn on the end of the horizontal than when drawn through the latter to make a cross, and shortest when drawn in one direction from the middle of the horizontal. A similar result emerged from a rough examination of 112 English school boys.

To the Müller-Lyer Illusion (Heyman's device) the Papuans were distinctly less susceptible than Europeans, as shown by the several statistical values taken together. All things considered, the Papuans were but slightly less constant and accurate in their observations than the English. Dr. Rivers thinks that the savages gave more exclusive attention to the lines themselves, the English to the figures as a whole. This will be seen to harmonize with other apparent facts supporting the view that the minds of savages are chained to particulars of sense, the latter being subordinated to the higher functions of apperception in civilized races. Other geometrical illusions from the Milton-Bradley set of 'Pseudoptics' were tried on the natives, who were also questioned *ad hoc* upon common phenomena of nature.

It is hoped that Dr. Rivers will have further opportunities for studying these Papuans until, for this group, statistical data of unques-

tionable significance be obtained, and that others will be stimulated to prosecute like investigations on other primitive groups. Enough has been said to indicate how futile it is to treat all savages as if they constituted a homogeneous class.

The last remark will be strengthened by a survey of the second paper, in which Dr. Rivers reports some results obtained, in part by Mr. D. Randall-MacIver, in larger part by himself, from the natives of Upper Egypt, who were principally typical *felläḥin*, speaking Arabic.

Without attempting any detailed comment upon their color vocabulary and the variability of usage, I proceed at once to summarize the main points, largely in the author's own words. "There was a very definite word for red, *aḥmar*, which was not only applied to objects which we should definitely distinguish as red, but also to colors such as orange, purple, violet and brown, which contain a red element. There was a somewhat less definite term for yellow, *aṣfar*, which was also used for orange and brown and was occasionally applied to green and faint red. The word for green, *akhḍar*, was still less definite, being very often applied to blue, violet, gray and brown. There was no definite word for blue," etc. The same word was indiscriminately used for blue, black and other dark colors. "The nearest approach to a word for blue was *läbäni*, milk color," used also for green, gray, brown. From all this "a decreasing definiteness in the nomenclature for color as one goes from red through yellow and green to blue, was as marked in these peasants of Upper Egypt as it is in the Papuans of Torres Straits and in so many other savage and semi-civilized races." "More variety in the terms applied to brown than to any other color," is noted, "over twenty different terms being given to brown papers and wools."

An interesting feature of the nomenclature was "the tendency to use words denoting differences of color-tone for differences of shade, *i. e.*, of luminosity" — "a peculiarity which is far from being associated * * * with absence of the color sense" as independently tested, yet which is analogous to the Homeric practice upon which Gladstone rested his hypothesis.

Color vision was tested with Holmgren's wools, Nagel's circles of colored dots and Lovibond's tintometer. These *felläḥin* reacted to the wools in all essentials precisely as had the Papuans of Torres Straits. A similar insensitiveness to blue was apparent. The resemblance between pink and violet, which were often compared, may

arise from this insensitiveness, which suggests a defect in Holmgren's test for ethnographic purposes, because 'a confusion of colors which in a European certainly means insensitiveness to red, may in other races be due to insensitiveness to (or lack of interest in) blue'; further, such linguistic defects as have been hinted at 'may be of influence in the process of matching, and may lead to confusions in this process which are in Europeans characteristic of color-blindness.'

To give some figures: Of 83 male natives examined, 4 (*i. e.*, five per cent.) were certainly color-blind, while others were doubtful and several gave indication of red-green weakness: this equals or exceeds the European average and contrasts strikingly with the condition found in Torres Straits. The results with the tintometer showed that the *felláhin*, like the English but unlike the Papuans, were somewhat less sensitive to red than to yellow, but were relatively insensitive to blue, like the Papuans and unlike the English.

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## NEUROLOGY.

*A Case of Astereognosis resulting from Injury of the Brain in the Superior Parietal Region.* William H. Teller, M.D., and F. X. Dercum, M.D. Journal of Nervous and Mental Disease, Vol. 28, No. 8, August, 1901. Pp. 459–461. With discussion by Drs. Charles W. Burr, James H. Lloyd, and William G. Spiller, before the Philadelphia Neurological Society, 25 February, 1901, reported in the same number of the Journal, pp. 468–470.

Taken together, these reports of cases and of the opinions of competent specialists constitute an excellent account of the condition called astereognosis, which consists in an inability to recognize objects by the sense of touch, that is, to feel the shape of objects by the muscular, joint and dermal senses.

The patient in this case was a negro aged twenty years. He was brought into the hospital after lying unconscious for a week from a blow on the head with a baseball bat. There was then an area of depressed skull in the left parietal region; complete paralysis of the right leg, right arm, right side of face and tongue.

Following the operation the facial paralysis soon disappeared; he could move his arm thirteen days after the operation, and two days later his leg, since which time voluntary control over his body has gradually improved.

His gait is slightly hemiplegic; he sways when standing with eyes closed; there is marked ataxia of movement of right arm, and decided hypesthesia on right half of body.

"With eyes closed the patient fails to recognize any object placed in his hand. * * * Slight hypesthesia to pin points, loss of space-sense, loss of the sense of location, hypalgesia and diminution of the pressure sense in the right wrist, hand, and especially the fingers, are observed. The perception of heat and cold is preserved, save that the responses are somewhat slow and the acuteness of perception slightly diminished. The knowledge of the position of the fingers in the right hand is also greatly impaired. Astereognosis is complete. * * * This case is extremely interesting, because the brain tissue was evidently injured in the region of the superior parietal lobule, especially in the region posterior to the motor area. Indeed, the conditions present in this case are *almost such as could have been desired in an experimental research in cortical localization.* The lesion appears to have involved the motor region but little as the resulting terminal hemiplegia is exceedingly slight. The astereognosis, on the other hand, is the most striking feature of the case."

GEORGE V. N. DEARBORN.

TUFTS COLLEGE.

*Ueber die Wirkung des Eucain B auf die Geschmacksorgane.* ARTHUR FONTANA. Zeitschrift für Psychologie und Physiologie der Sinnesorgane, XXVIII., 253–260.

Signor Fontana, a medical student at Turin University, adds the results of a series of experiments with eucaine B to the scanty data thus far obtained in regard to the effect upon the taste-organ of the various anæsthetics employed in therapeutics. Of these substances, the effect of cocaine and of gymnemic acid (a drug derived from the East Indian *gymnema sylvestre*) had alone been tested. Eucaine B is the hydrochloric acid salt of benzoylvinyldiacetonalkamin. As a material for taste experiments it possesses over cocaine the advantages of lower price and of slighter toxic properties and of furnishing solutions which can be sterilized by heat without decomposition and which retain their properties for a considerable time. Signor Fontana has made with this substance a series of experiments upon himself with control experiments upon Dr. Kiesow and upon four other persons. The anæsthetic was in aqueous solution and was applied with a brush of about 6 mm. in diameter to the tip and to the forward part of the sides of the tongue. The intensity of the anæthesia was measured by the number of brush-strokes, and by the saturation of the solutions,

which were of five-tenths of one and two per cent., and of complete saturation. The test stimuli regularly employed were graduated series of aqueous solutions of sulphate of quinine, cane sugar, common salt and tartaric acid. Three drops at a time of the material at a temperature of 15°–18° C. were applied to the tongue with a pipette.

Signor Fontana's results are as follows: Eucaine B, like cocaine, tends to raise the intensive limen for all four of the taste-qualities. Its effect, like that of cocaine, is far more marked upon bitter and sweet than upon salt and sour, and is most marked upon bitter. The degree of effect of eucaine B upon the three qualities less implicated can be stated in each individual case. Fontana does not compare eucaine B with gymnemic acid. In this connection, however, one may recall the showing of Dr. Kiesow that gymnemic acid, like cocaine, affects sweet and bitter much more than salt and sour, that, unlike cocaine, it affects sweet more than bitter, and that its influence upon bitter is much less intensive than the effect of cocaine upon sweet. Thus in eucaine B and gymnemic acid we have two substances which may eliminate one quality and yet at the same time may produce slighter and mutually comparable effects upon the other three.

Fontana tested his sensibility for the different qualities three minutes and five minutes after the application of the anæsthetic and again at intervals of five minutes until the normal limen was restored. From these trials it appears that the effect of eucaine B is less lasting than that of cocaine.

E. A. McC. GAMBLE.

WELLESLEY COLLEGE.

## EXPERIMENTAL.

*Ueber den Einfluss der Farbe auf die Grösse der Zöllnerschen Täuschung.* VITTORIO BENUSSI. Zeitschrift f. Psychologie u. Physiologie d. Sinnesorgane, Bd. XXIX., Heft 4 u. 5, S. 264–341; Heft 6, S. 385–433.

As is indicated by the title, the purpose of the experiments was to determine the effect of color (and brightness) upon the Zöllner illusion. The apparatus consisted of a perpendicular frame, covered, usually, with white cardboard, and having the figure drawn upon this as a background. The single main-line was fourteen centimeters long, with transversals five and one-half centimeters long, and crossing at an angle of twenty degrees. At the upper end of the main-line a thread was attached, which ran over the top of the background, and was weighted behind in order to keep it drawn tightly. The test consisted in adjusting the position of the thread so as to appear to be

in line with the main-line of the figure. The amount of deflection necessary to produce this appearance was read off from a millimeter scale at the top of the frame over which the thread passed. Seven one-colored figures were used (red, yellow, green, blue, violet, gray, and black), and forty-two combinations of these into two-colored figures, in which the transversals and main-lines were colored differently.

In one-colored figures the illusion varies with the brightness difference between the figure and background, being weakest for the light gray and strongest for the black. The other one-colored figures come in between the black and white in the order yellow, red, green, violet, and blue. In the two-colored figures a darkening of the transversals and an increase in the brightness of the main-line result in a greater illusion. The illusion also varies directly with the brightness difference between the figure and background, decreasing with decreasing brightness difference. The illusion was also found to be reduced to a minimum in those two-colored figures which have a maximal brightness difference between main-line and ground and a minimal brightness difference between transversals and ground. In colored figures, then, the force of the illusion depends upon the brightness difference between the figure and background and the color quality of the components of the figures. The equations and laws for this brightness difference between the figure and background are worked out to a very considerable length.

It was found that, in order to produce a maximal illusion, the transversals must cross at an angle of from twenty to thirty degrees; they must be relatively long and close together; there must be a maximal brightness difference between the transversals and background and a minimal brightness difference between the main-line and background.

In determining whether or not the illusion depends upon eye-movement, it was found that there was practically no difference in the results when the eyes were fixated upon the point of junction of the thread with the main line and when they moved freely. Eye-movement alone was regarded as insufficient to account for the illusion. (The author assumes that steady fixation eliminates eye-movement; it would seem that this assumption is not entirely accurate.)

With reference to the nature of the illusion, the hypothesis that it is an illusion of judgment is dismissed as unsatisfactory. In support of this statement experiments were made with the two parts of the regular figure united by means of the haploscope. If the illusion

were really one of judgment, it would not be affected by this method. The results of over fifty-five hundred separate determinations demonstrated that the illusion is decreased when the two parts of the figure, the main-line and the transversals, are united by the haploscope, the illusion, however, still varying with the brightness difference between the figure and background.

MABEL CLARE WILLIAMS.
UNIVERSITY OF IOWA.

*Untersuchungen über die sogenannten Aufmerksamkeitsschwankungen.* E. WIERSMA. Zeitschr. f. Psy. u. Phys. d. Sinn. Bd. 28, H. 3–4. Pp. 179–198.

In this article Dr. Wiersma continues his researches upon the attention waves and extends them to include the effects of changing conditions upon the capacity for perception as measured by the time of visibility of a faint stimulus. Again the three senses, sight, touch and hearing, were investigated, and each with respect to the effect of the time of day, of mental and physical fatigue, and of the use of alcohol and sodium bromide.

The change during the course of the day was found to be dependent upon whether the individual was a morning or evening worker. For a morning worker the period of greatest perceptive capacity was at noon, and the early morning hour was considerably better than the evening hour. For the evening worker, on the contrary, there was a gradual increase in capacity from morning to evening. The experiments were taken three times a day only, between nine and ten in the morning, two and three in the afternoon, and seven and eight in the evening, so that the exact point of change between increasing and decreasing efficiency could not be established. It is suggested that the difference in type is rather a matter of habit than inheritance, for experiments upon nurses who had no fixed periods of mental work showed no such marked influence of the time of day as did the experiments upon the academic men.

Bettmann's results were confirmed in the experiments upon the influence of mental and bodily fatigue. The period of visibility of the minimal stimulus was decreased by both mental and physical work, adding or walking for two and a half hours, although the decrease was more marked for the mental than for the physical work.

Ten c.c. of alcohol had from the beginning a marked effect in decreasing ability of perception, while three grams of sodium bromide had just as marked an effect in increasing it. The effect of the bromide was also found to persist for three days after the drug had been taken.

Dr. Wiersma's experiments show that the attention wave furnishes a very delicate and convenient method of measuring mental effectiveness under many influences, and suggest that further investigations along the same line should prove fruitful. W. B. PILLSBURY.

## CHILD STUDY.

*The Observation of School Children.* JOHN A. HANCOCK. Pedagogical Seminary, September, 1901. Pp. 290–340.

This article professes to be an outline of a course in the experimental study of children for use in training and normal schools. The whole field of child study is covered, in a way, but the classification employed is careless; *i. e.*, we find the sub-title *motor tests* under the title *instructions for eye examinations.* Quite a full account of how and what to do in the way of physical measurements is given; some space is devoted to methods of observing fatigue and nervous disorders; mental ability is treated under the heads of memory, association and discrimination, followed by an elaborate statement of the questionnaire method taking up nineteen pages, about three-fifths of the entire article. The presentation of the questionnaire method is merely a formulation of questions implying the results of the principal researches of that character so far published from Clark University. One can not but regret that this method is emphasized so disproportionately to the more direct quantitative tests of mental ability. The whole article is little more than a catalogue of what has been done and shows lack of sifting and critical selection in respect to contents; it is as if one should take up all the recorded attempts at problems in physiology, and base demonstration experiments upon them without attempting to organize the material into a scientific presentation of the subject. Of this the author himself seems aware, and the chief value of the article is that it truly represents the chaotic state of this phase of genetic psychology. The time is ripe for a master mind to seize upon the heterogeneous mass of material at hand and give us a science.

CLARK WISSLER.

NEW YORK UNIVERSITY.

## ETHICS.

*Die Entwicklung der Ethik. Vortrag gehalten vor der Aristotelian Society of London.* DR. JAMES LINDSAY. Uebersetzt von Ludwig Busse. Zeitsch. für Phil. u. phil. Kr., April, 1901, pp. 190–210.

The author expressly limits his accounts of the development of ethics to its last stage, during which 'the moral consciousness, under

the influence of philosophy and humanistic tendencies, attains to its maturity.' But the address in fact pays but scant attention to the development of ethics prior to the middle of the nineteenth century, two sketchy pages containing all that is said. And the ethics of the last fifty years is not considered developmentally. Dr. Lindsay in fact has two main purposes in this article, to sum up what has been done and left undone for ethics by studies owing their inspiration to the evolutionary point of view, and to insist with fervor that ethics must rest on metaphysics and theology as its only secure foundation.

The author is at pains to be fair and open minded in his estimation of evolutionary ethics, and most of his positive and negative criticisms are just, though they have been made before. But in some of his statements, *e. g.*, when he says that scientific ethics does not esteem itself a normative science (p. 203), and when he repeatedly declares that the value of moral principles is wholly unaffected by their origin and history, he voices his own views, or at most those of his school, and not, as he would have us believe, the views all authorities have come to accept. Moreover, Dr. Lindsay fails to mention the most important services of evolutionary ethics. He seems quite unaware that when morality is observed and its achievements noted, first in its simplest initial form, and then as each successive complication appears down the course of its history, nature presents us, as Professor Dewey has well shown, with analyses and syntheses quite similar to those performed in scientific laboratories, and of like scientific value. And, returning to the point mentioned above, it is precisely in throwing light on the function and value of moral principles that evolutionary ethics performs its greatest service. Dr. Lindsay would hardly maintain that his rôle in life and his value as a man depend wholly on what he has been and done during the last week. And yet he would have us judge morality by its 'present,' which in relation to its life history is shorter than a week in relation to his.

The plea for a metaphysical ethics is based essentially on the existence of metempirical facts of fundamental moral importance, and an ethics that omits these from its foundations is declared to be inadequate. And inadequate a naturalistic ethics undoubtedly is. But Dr. Lindsay forgets that his metempirical 'facts' are far from being fully authentic or even generally accepted. He might have been less ready to scorn naturalistic ethics as a torso, had he reflected that his metaphysical statue may possibly be but a mass of crumbling dust.

S. E. Mezes.

University of Texas.

## NEW BOOKS.

*Das psychotische Moment.* M. FÜHRMANN. Leipzig, Barth. 1903. Pp. 95.

*Abhandlungen zur Physiologie der Gesichtsempfindungen.* H. 2. J. VON KRIES. Leipzig, Barth. 1902. Pp. 197.

A collection of papers reprinted by Professor von Kries from the Zeitschrift für Psychologie.

*Esquisse psychologique des peuples Européens.* A. FOUILLÉE. 2ème éd. Paris, Alcan. 1903. Pp. xix + 550.

*The Mind of Man.* A Text-book of Psychology. G. SPILLER. London, Sonnenschein; New York, Macmillans. 1902. Pp. 552. $2.75.

*L'Etica evoluzionista; studio sulla filosofia morale di Herbert Spencer.* G. SALVADORI. Turin, Bocca. 1903. Pp. xv + 479. L. 10.

*Aristotle's Psychology.* Translations of the De Anima and Parva Naturalia, with introduction and notes. W. A. HAMMOND. London, Sonnenschein; New York, Macmillans. 1902. Pp. lxxxvi + 339. $3.00 *net.*

*Hegel's Logic.* An Essay in Interpretation. J. G. HIBBEN. New York, Scribner. 1902. Pp. x + 313.

*Studies in the Cartesian Philosophy.* N. SMITH. London and New York, Macmillans. 1902. Pp. xiv + 276. $1.60 *net.*

*Investigations in the Department of Psychology and Education of the University of Colorado*, Vol. I., No. 2. ARTHUR ALLIN, Editor. University of Colorado, Boulder, Colorado. 1902.
Contains an important paper on 'The Survival Values of Play,' by H. A. Carr, which will be summarized in a later issue.

*Les Obsessions et la Psychasthénie*, I. PIERRE JANET. Publications from the Laboratory of Psychology of the Clinic of the Salpêtrière (3ᵈ series). Paris, Alcan. 1903. Pp. xii + 743. Fr. 18.

The following publisher's note (condensed) gives a description of this important volume. We translate it as a preliminary notice. "M. Janet gives in this volume a new application of the method used by M. Ribot and employed by himself earlier in his work on 'Neuroses

and Fixed Ideas.' The method consists in drawing from psychology all the light which it can afford on the classification and interpretation of the facts of mental pathology, and in seeking reciprocally, in the abnormal alterations of mind, those natural data of observation and experiment which further the analysis of human thought. It thus has equal interest for psychology and for medicine. Among the phenomena which are treated in this study are the following: obsessions, abnormal impulsions, mental diseases proper, the doubting mania, ticks, emotional agitations, phobias, and certain detailed phases of disease, such as neurasthenia, unusual feelings of strangeness, and derangement of personality. M. Janet applies to those having the affections which come under these heads the term 'scrupuleux,' because a certain 'scruple' constitutes one of their essential characters, and also by the more precise name of 'psychasthéniques,' which appears to him to sufficiently describe the enfeeblement of the psychological functions which are involved. The volume consists of two parts. The first is descriptive and analytic, under the title 'Analysis of Symptoms.' In it we find the treatment of obsessions, various uncontrollable agitations, psychasthenic stigmata, etc. In the second, which is more historical and general, the author gives us general studies on the weakening of psychological tension, the evolution, diagnostics, and treatment of psychasthenia and its place among nervous conditions. By the comparison of the different symptoms in one book, he makes a contribution to the study of diagnostics, prognostics, and treatment of the affections included, which play an important role in nervous pathology; and further gives a psychological analysis of these different phenomena so far as he has been able to discover among them recognized characters and to arrive at an interpretation which allows the grouping of the greatest possible number of facts under general conceptions. The volume, like the preceding one on Fixed Ideas, will be followed by a second part, published in collaboration with Professor Raymond, which will comprise clinical observations on a great number of diseases and will include psychological and clinical descriptions and documents affording the justification and the proofs of the interpretations presented in this volume by M. Janet."

*The Jewish Encyclopedia, I. A–Apo.* Is. SINGER, Man. Editor. New York, Funk & Wagnalls. 1901.

A work of encyclopedic character, devoted to the history of religion, literature, and customs of the Jewish people from the earliest times to the present day — prepared by a corps of scholars and spe-

cialists under an editorial board, Dr. Isidore Singer being the managing editor. This encyclopedia promises to be, judging from the first volume, an extremely comprehensive and valuable work. It falls to our province to notice only the philosophical and psychological articles, together with the biographies of philosophical thinkers. These we find, as represented in the present volume, to be thorough and accurate. We commend the encyclopedia in general terms, at the same time reserving for a later notice, after subsequent volumes appear, a more detailed appreciation of the philosophical articles. Among the biographical articles, beginning with the Jewish writers and men of genius of the earliest times and coming down to those of the present, we notice the inclusion of contemporary writers, both in England and America. The long list of Alexanders, for example, includes the name of our esteemed contemporary at Manchester, Professor Samuel Alexander.

J. M. B.

*Les obsessions et les impulsions.* PITRES and RÉGIS. Bib. intern. de Psych. Expér. de Toulouse. Paris, Doin. 1902. Pp. 434. 4 fr.

We have from time to time listed in this department the successive issues of the International Library of Experimental Psychology, edited by Dr. Toulouse. The separate volumes, of which some forty are projected, are now rapidly appearing and we have nine of them already upon our table. While expecting to notice, in considerable detail, the individual volumes, it may be well to call the attention of our readers to the enterprise as a whole, to the comprehensiveness of the undertaking, and to the high character of the separate volumes.

J. M. B.

*Ethics of T. H. Green, Herbert Spencer and J. Martineau.* H. SIDGWICK. London and New York, Macmillans. 1902. Pp. xli + 374.

These lectures of Professor Sidgwick have been edited by Miss E. E. Constance Jones, of Girton College, Cambridge, from manuscript which was not prepared for publication by Professor Sidgwick, and, consequently, did not have his final revision. They are complete, however, as he delivered them, and constitute a most interesting and valuable critical volume from the pen of Professor Sidgwick. The essay on Green is perhaps the most unsympathetic of the group, Sidgwick seeming to have some difficulty in entering into the spirit of the Oxford philosopher. On the other hand, that on Martineau seems to the present writer illuminating and valuable. Mr. Spencer is treated in ten lectures in considerable detail and with that quality of judi-

cial fairness which we are accustomed to expect from Professor Sidgwick when at his best. This part of the volume — that devoted to Herbert Spencer — seems to be a very valuable addition to the critical literature of evolution ethics in general. One reflection forces itself upon the mind as we follow Professor Sidgwick: it is that whatever we may find in the criticism made by other writers, of systems with which they do not agree, in the way of lack of information and inadequate representation of the works criticised, such faults did not attach to the work of this critic. The moderation, balance, and reserve of Professor Sidgwick's mental appreciations show themselves here in all their force. J. M. B.

*L'Année Philosophique.* F. PILLON. 12th year, 1901. Paris, Alcan. 1902.

In this volume M. Pillon continues his annual critical summary of the literature of French philosophy for the year 1901. The volume also contains a list of the writers referred to, which constitutes a selective bibliography of French philosophical literature for the year.

*Outlines of Psychology.* W. WUNDT. Translated by C. H. Judd. Second edition, revised from the fourth revised German edition. New York, Stechert. 1902. Pp. xxii + 390. $2.00.

In the translator's preface to the English edition, the translator points out by page citation the most important additions to the text which have been made in the work as contrasted with the first English version. Among these additions are various literary citations which Professor Wundt himself inserted in the fourth German edition. These constitute a select bibliography made by the author. The translator adds that he has not found it necessary to make any significant changes in the terminology adopted for the earlier edition. A brief notice of the first English edition is to be found in an earlier number of this REVIEW (V., 1898, 208). J. M. B.

*Crimes et Anomalies mentales constitutionnelles.* A. FOREL and A. MAHAIM. Paris, Alcan. 1902. Pp. 300.

Mainly a report on certain French medico-legal cases with citation of 'expert' opinions.

*Philosophie générale et Métaphysique.* Vol. I. of the Proceedings of the International Congress of Philosophy, Paris, 1900. Paris, Colin. 1900. Pp. xxii + 460. 12.50 fr.

This is Vol. I. of the detailed proceedings of the congress. Subsequent volumes are to be devoted to Ethics (II.), Logic and History

of Science (III.), and History of Philosophy (IV.). The whole will constitute a very valuable collection of papers covering the entire field of philosophic thought and serving as a worthy monument, in this particular branch, of the great Exposition of 1900.

J. M. B.

*Talks to Students on the Art of Study.* FRANK CRAMER. San Francisco, Hoffman-Edwards Company. 1902. Pp. 309.

This book is by the author of that interesting study 'The Method of Darwin,' published several years ago. This volume is devoted, as its title indicates, to talks upon certain psychological topics which are most vital to true pedagogical method, such as habit, observation, attention, memory, etc. The book seems to us to be sound and also interesting — one which the teacher will find instructive and helpful.

J. M. B.

*The Faculty Corner.* Papers by the Faculty of Iowa College. Grinnell, Iowa. 1901. Pp. 208.

This book is of interest to students of philosophy because of its tribute to the late Professor James Simmons, to whom it is dedicated. A paper called 'Education According to Nature,' written by Professor Simmons, is included in it. Other essays, to which it may be well to refer, are on 'The Usefulness of Art,' 'The Study of Society,' 'The Elements of Enjoyment in Music,' 'Utility as a Criterion of Being' — all by professors of Iowa College. J. M. B.

*The Founder of Mormonism.* I. W. RILEY, with an introduction by Professor G. T. Ladd. A psychological study of Joseph Smith, Jr. New York, Dodd, Mead & Co. 1902. Pp. xix + 426.

*The Force of Mind, or the Mental Factor in Medicine.* ALFRED T. H. SCHOFIELD, M.D. Philadelphia, Blakiston. 1902. Pp. xiv + 309.

*Personal Idealism; Philosophical Essays by Eight Members of the University of Oxford.* HENRY STURT, editor. London and New York, Macmillans. 1902. Pp. ix + 393.

A volume devoted to what the authors call 'Personal Idealism,' from the general tendency of all the essays to emphasize personality as a fundamental category in philosophical construction. The essayists comprise Stout, Schiller, the editor, and others less well known. We hope to print a review later.

*The Philosophy of Religion in England and America.* ALFRED CALDECOTT. New York, Macmillans. 1901. Pp. xvi + 434.

A general classification, résumé, and criticism of writings on the philosophy of religion. The author's scheme is extremely comprehensive, dealing not only with works purely philosophical and psychological (the latter less effectively), but also with those which may properly be called theological. It covers ground which has not been gone over in the same systematic way before. We notice particularly the attempt to include American writers as well as English, the appendices being devoted especially to three American writers, who were evidently unfamiliar to the author at the time his main manuscript was composed. J. M. B.

*Nineteenth Annual Report of the Bureau of American Ethnology, Parts I., II., 1897–1898.* J. W. POWELL, Director. Washington, Govt. Pr. office. 1900. Pp. 1160.

This report contains a long essay on 'Esthetology' (the 'science of activities designed to give pleasure') by the lamented Director Powell, which will be valued as one of his latest writings. Dr. W. J. McGee contributes a paper on 'Primitive Numbers,' and Dr. Cyrus Thomas one on 'Numeral Systems of Mexico and Central America.'

*The Principles of Logic.* H. A. AIKINS. New York, Holt, 1902. Pp. x + 489.

*L'Association des Idées.* E. CLAPARÈDE. Bib. Intern. de Psych. Expér. Paris, Doin, 1903. Pp. 426. 4 fr.

*An Essay on Laughter.* J. SULLY. London and New York, Longmans, 1902. Pp. xvi + 441.

*Can Telepathy Explain?* M. J. SAVAGE. New York and London, Putnams, 1902. Pp. xvi + 243.

In their advertisement the publishers say: "Dr. Savage * * * here states a great number of well-authenticated instances of apparently spiritistic revelation and communication." In the preface Dr. Savage says: "I frankly confess that I am strongly inclined to hold the belief in continued personal existence as capable of proof and in the possibility of at least occasional communication." In the appendices he gives 'some [favorable] opinions of well-known men'; 'a partial list of names favoring * * * occasional communication' (good, bad and indifferent, *e. g.*, Joseph Jefferson, Queen Victoria, and 'the parents of General Grant'! Where is that humorist, Mark Twain?); 'a partial list of books * * * nearly all in favor of the spiritistic theory' (*e.g.*, 'Planchet, or the Despair of Science,' 'Gates Ajar,' and 'Fact and Fablein Psychology'!) J. M. B.

*Annual Report of the Department of the Interior: Commissioner of Education, I.* Washington, Govt. Ptg. Office. 1902. Pp. cxii.

This report contains the usual summary of educational progress for the year, by Commissioner Harris, and a valuable list of publications of the Bureau of Education from 1867 to 1902 (with indications as to which of the titles are out of print). The body of the Report is made up of documents relating to education at home and abroad.

*Lehrbuch der Psychologie.* F. Jodl. Second Ed., 2 vols. Stuttgart and Berlin, Cotta'sche Buchhandlung, 1903. Pp. xx + 435, and x + 448.

## NOTES.

We are informed by Professor W. R. Sorley, of Cambridge University, that Professor Robert Adamson has left a considerable body of manuscripts, which he is editing for publication. He expects that there will be two volumes, chiefly psychological, entitled, 'The Development of Modern Philosophy.'

We notice the appearance of the first number of the new *Hibbert Journal* — a review devoted to philosophy and the philosophical treatment of religion. Among the coöperators, the name of Professor Howison, of the University of California, appears. The journal is published by Williams & Norgate.

Parts I. and II. have appeared of the *Journal für Psychologie und Neurologie*, which is a continuation, beginning at Volume XI., of the *Zeitschrift für Hypnotismus*. The managing editors are Professors August Forel and O. Vogt. It is published by Barth of Leipsic.

It is announced in the press that Professor W. Knight has resigned the chair of Moral Philosophy in the University of St. Andrews.

The Carnegie Institution has granted $1,600 to Professor E. W. Scripture, of Yale University, for prosecution of researches on the voice.

We are informed that the issue of Wundt's *Philosophische Studien* has been closed with Vols. XIX. and XX., which were dedicated to Professor Wundt on his seventieth birthday. This journal will appear in the future under the title *Archiv für die gesammte Psychologie*, under the direction of a board of editors comprising, besides Professor Wundt himself, Professors Külpe of Würzburg, Kirschmann of Tor-

onto, Kraepelin of Heidelberg, Meumann of Zurich, and Dr. Wirth of Leipzig, Professor Meumann being Editor-in-chief.

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OWING to delays in the printing of *Studies from the Harvard Psychological Laboratory* (No. 17 of the Monograph Series of the REVIEW), Monograph No. 19 has been sent out without waiting for the appearance of the preceding numbers. It is expected that Monographs 17, 18, and 20 will appear in due succession; they will be sent to subscribers of the Monograph Series as they are ready.